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VOTER OPINION AND INVOLVEMENT THROUGH CIVIC EDUCATION (VOICE)

The *VOICE* Impact Evaluation in the Democratic Republic of the Congo Final Report

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The *VOICE* Impact Evaluation in the Democratic Republic of the Congo: Final Report

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Executive Summary

This document reports the results of the *Voice Impact Evaluation*, a study conducted by IFES to assess the effects of its *Voter Opinion and Involvement through Civic Education Program* (*VOICE*) in the Democratic Republic of the Congo. The *VOICE* program was designed to improve the capacity of the Congolese people to participate effectively in the ongoing process of decentralization in the DRC. This process, mandated by law in 2007, calls for the reorganization of the country's administrative units, and increases in the powers of Local and Provincial governments in order to bring greater accountability and inclusion of subnational units in the country's political, economic, and social development.

VOICE consisted of a range of activities aimed at enabling ordinary Congolese citizens to better understand and engage in the decentralization process, motivating individuals to participate in government and politics more generally, and providing community-based organizations the capacity to implement civic and voter education campaigns. The focus of this evaluation is on the central civic education-related activity of the *VOICE* program, the *Boîtes à Images* sensitization sessions in which facilitators from local community-based organizations presented community audiences with a variety of images that were designed to stimulate discussion and learning about decentralization and, to a lesser extent, broader issues of political, economic, and democratic development. These sessions, lasting approximately two hours with audiences of approximately 100 individuals, were conducted throughout 2010 and 2011 in four target provinces --- South Kivu, Maniema, Kantanga, and Bandundu. Due to budget limitations, our evaluation is limited to assessing the impact of the *Boîtes à Images* sessions conducted during the summer of 2011 in Bandundu province.

The evaluation was designed primarily to determine the extent to which the *Boîtes à Images* sessions led to changes in ordinary citizens' knowledge and attitudes about decentralization and democratic politics more generally, and whether the sessions prompted greater cognitive and behavioral engagement with the Congolese political process. Moreover, it sought to assess the conditions under which the sessions had greater or lesser impact, focusing on a number of factors that potentially affect the impact that the *Boîtes* sessions had on individuals, including the quality of the instruction and organization of the event, the length of time since the *Boîtes* session took place, and the individuals' own prior political, social, and informational resources. Finally, the evaluation sought to determine the relative magnitude of *direct* effects on individuals who may have attended the sessions, as well as *indirect* or "spillover" effects on non-attendees in treatment villages who may have learned about decentralization issues through discussion with others in their social networks who did attend the *Boîtes* events.

To accomplish these task, we implemented a complex research design with two components: first, a comparison between respondents from 8 villages in Bandundu province where *Boîtes à Images s* sessions took place and 8 villages from similar "control villages" in neighboring *groupements* where sessions did not take place; and second, a field-experimental "encouragement design" whereby 100 individuals in each of 8 villages were randomly encouraged at the end of a baseline interview to attend the upcoming *Boîtes* session, while 40 randomly-selected individuals in those "treatment villages" received no information about the workshop. Follow-up interviews were conducted with all respondents between one day and one month after the workshops in the treatment villages took place. The study is one of the first evaluations of adult civic education programs to make use of experimental methods, which

provide greater confidence in the attribution of causal effects than has been possible in the mostly passive observational designs in previous research in the field. To our knowledge, it is the first that implements an "encouragement manipulation", one that preserves the benefits of experimentation in a situation where it is not possible to randomly assign individuals to experience a treatment, nor to restrict exposure to those specifically chosen individuals.

The study points to a series of important findings about the kinds of effects that the *Boîtes à Images* sessions had, and the conditions under which the sessions had their strongest impacts. There were *large* differences between individuals in control and treatment villages in terms of their changes in factual knowledge about decentralization before and after the *Boîtes* events took place in the treatment villages. While respondents in control villages increased to some extent on decentralization-related knowledge (with the percentages of correct responses increasing between 5-10% for most questions), respondents in treatment villages increased at substantially higher rates (between 12 and 45% increases in correct responses for each question). On an overall scale of "correct decentralization knowledge" (ranging from 0 correct to 6 correct answers), the average respondent in control villages increased by only .45 correct answers over time, while the average respondent in treatment villages increased by 1.5 correct answers from the pre- to post-*Boîtes* interviews. These effects remains strong in the context of regression models where the treatment and control samples are carefully balanced on possible confounding variables, and in models that control for the individuals' pre-*Boîtes* levels of the decentralization knowledge. These effects are impressive, especially given that they stem from a single civic education event conducted at the village level. To the best of our knowledge, these are some of the largest effects registered in the several adult civic education evaluation studies previously conducted, and certainly the largest registered in these studies on political knowledge (Finkel 2003; Finkel and Smith 2011; Finkel *et al.* 2012).

We also found village-level effects of the *Boîtes* sessions on other knowledge-related items, such as the individual's general knowledge about DRC political institutions and actors, and on the individual's *perception* of their knowledge about decentralization and the DRC constitution. These effects were of smaller magnitude than those observed on the general decentralization knowledge scale, but they do indicate that the *Boîtes* sessions were beneficial to some degree in stimulating *general* political learning, and in stimulating greater confidence among individuals in their own store of decentralization and constitutional knowledge.

However, we found few corresponding effects on individual attitudes about, or support for, the decentralization process. There were no differences between the changes in support for decentralization among respondents in control villages and respondents in treatment villages, no differences between treatment and control respondents in terms of their perceptions of the "good" and "bad" features of decentralization, or their satisfaction with the current pace of the decentralization process. Individuals in treatment villages, moreover, tended to change slightly in the direction of greater, not lesser, national authority in a series of policy areas (roads and transportation, schools and education, police and military, taxation), compared to comparable individuals in control villages. There were also no effects of the *Boîtes* session on more general orientations about democracy, on political interest, discussion, or efficacy, nor on political participation. The effects of the *Boîtes* events appear to have been more or less exclusively concentrated in the realm of decentralization and general political *knowledge*, with no concomitant effects on decentralization attitudes or other political orientations or behaviors.

We exploited the "randomized encouragement" feature of the design in order to determine the direct effects of attending the *Boîtes* event among individuals in treatment villages. Using a variety of "intent to treat" and "instrumental variables" models to overcome possible biases related to self-selected exposure to the treatment, we found consistent evidence that *Boîtes* attendees registered large and significant gains on decentralization knowledge, relative to non-attendees, with these effects being even larger than the overall village effects mentioned above. *Boîtes* attendees also were found to have gained significantly on the other variables (general knowledge and perceptions of decentralization and the DRC constitution, support for national authority in roads, police and military affairs) shown to be significant at the village level as well. Further analysis showed minimal "spillover effects" from attendees to others in their family, neighborhood or friendship networks. Taken together, the results suggest that virtually all of the village-level impacts on the knowledge and decentralization orientations were concentrated among those who attended the *Boîtes* sessions. It seems reasonable to speculate that the single, relatively brief *Boîtes* exposure was not sufficient to stimulate the kind of post-treatment diffusion via interpersonal discussion necessary for spillover effects to occur, especially given the breakdown of the decentralization process at the elite level and its relatively low salience in the DRC at the time the study was conducted.

We further examined the conditions under which the *Boîtes* sessions produced larger impacts. First, the *quality of the Boîtes* session mattered a great deal. Events with higher reported satisfaction of the participants regarding the organization, information delivery and competence of the facilitators showed substantially stronger effects on decentralization knowledge and many of the other knowledge-related items of lesser perceived quality. Importantly, individuals who attended *the highest quality events* often showed significant increases in the cluster of orientations related to political engagement: interest, efficacy, political discussion and participation, relative to comparable individuals in control villages. As was found in previous research, however, the numbers of individuals in DRC treatment villages who were trained at the highest levels of session quality were relatively small. Thus, the results indicate both the strong *potential* for effects from high quality civic education, as well as the more limited *actual* impacts the sessions had on decentralization and political engagement outcomes.

Second, the *time since the event* mattered as well. Treatment village respondents interviewed closer to a month after the *Boîtes* event showed substantially less improvement in decentralization knowledge, relative to comparable individuals in control villages, than respondents interviewed in days immediately following the event. Treatment effects decline by over 60% from the day after the event to 26 days later on the key variable of decentralization knowledge, with even steeper declines on the other knowledge-related political engagement orientations. This pattern is consistent with a "fade-out" effect, such that individuals are forgetting after several weeks a large portion of what they may have learned in the days following the treatment. We cannot show this conclusively with the data at hand, however, since individuals interviewed at different times after the sessions had been exposed to different treatments. Nevertheless, this pattern shows strong support for the notion — consistent with previous work — that the single-shot *Boîtes* treatments may not have produced lasting impact on the orientations they were designed to affect.

Finally, we found no consistent evidence that the effects of the *Boîtes* sessions differed across individuals in treatment villages with different demographic, political, or social characteristics. There were similar effects of the *Boîtes* session for men and women, for individuals with

different educational attainment, for individuals with varying levels of secondary group membership, and for individuals who had and had not lost family members in the recent DRC armed conflict. However, we did find suggestive evidence that the *Boîtes* treatment had greater effects on those with lower initial levels of decentralization and general political knowledge. In every instance, knowledge gains from the *Boîtes* events were greater among those with the greatest "need" for the information.

Based on these findings, we propose the following recommendations for future civic education design, implementation, and evaluation in the DRC:

- *Future civic education programs should continue to use "Boîtes à Images" and other visual materials*.
- *Future programs must go beyond "one-off" village-level treatments in order to maximize individual level impact.*
- *Future programs must give greater emphasis to the training of facilitators and to improving the general organization of civic education treatments.*
- *Future programs should emphasize post-treatment discussions of participants to maximize the possibilities of spillover effects.*
- *Future evaluations should continue to exploit experimental designs in order to make more credible causal inferences about program effects, while recognizing their potential limitations in certain settings as well.*

Chapter I. The *Voice* Impact Evaluation in the Democratic Republic of Congo

A. Study Introduction and Goals

This document reports the results of the *Voice Impact Evaluation*, a study conducted by IFES to assess the effects of its *Voter Opinion and Involvement through Civic Education Program* (*VOICE*) in the Democratic Republic of the Congo. The *VOICE* program was designed to improve the capacity of the Congolese people to participate effectively in the ongoing process of decentralization in the DRC. This process, mandated by law in 2007, calls for the reorganization of the country's administrative units, and increases in the powers of Local and Provincial governments in order to bring greater accountability and inclusion of subnational units in the country's political, economic, and social development.

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These goals were accomplished by conducting interviews (through *BERCI*, a respected Congolese survey company) with a random sample of 1,120 individuals in 8 "treatment" villages where *Boîtes à Images* sessions took place, and comparing their responses to those of a random sample of 480 individuals in 8 comparable "control" villages where *Boîtes* sessions did not take place. Baseline interviews were conducted with all respondents before the sessions took place in treatment villages, and all respondents were re-interviewed some days or weeks after the *Boîtes* sessions took place. The survey instrument contained an extensive battery of questions to measure the individuals' factual knowledge of the decentralization process and of political

institutions in the DRC, support for decentralization and for changes in the authority of local governments in various policy areas, support for democratic values and processes, perceived political knowledge and competence, and levels of political discussion and participation. In "treatment" villages, individuals were also asked in the follow-up interview about whether they had attended the *Boîtes à Images* session that had recently taken place, and, if so, about their experiences and satisfaction with the event.

The study also represents a major advance over previous work evaluating the impact of adult civic education programs in developing democracies that have been conducted by the author and others over the past decade and a half (e.g., Bratton *et al.* 1999; Finkel, 2002; 2003, Finkel and Smith 2011, Finkel *et al.* 2012). All of the previous evaluations of civic education impact have been based on observational research designs, i.e., with data collected on non-randomized treatment and control groups, often at one point in time long after the activities have taken place. These studies have provided valuable information, but the observational nature of the designs means that one cannot rule out the possibility that the "self-selection" of individuals into the civic education workshops led to biased inferences about the causal effects of the treatments. The current study includes a *randomized experimental component*, whereby randomly-selected individuals in the treatment villages were "encouraged" in the baseline interview to attend the upcoming *Boîtes* event. As we discuss in chapter II, this experimental manipulation gives us leverage with which to make inferences about the effects of exposure to the *Boîtes* sessions with much greater confidence than has been possible in previous civic education research (for other field experimental work on civic-education related topics, see Paluck and Green 2010, Fafchamps and Vicente 2010; Collier and Vicente 2009).

The report is organized as follows. In the remainder of this chapter, we discuss the content of the *Boîtes à Images* "treatment" and our framework for analyzing its potential effects. In Chapter II, we discuss the study's methodology, including the sampling design, the experimental manipulation, the survey instrument and details about the field work, and the statistical methods that we utilize to estimate treatment effects. Chapter III presents the results of the analyses. Chapter IV concludes and draws implications for the implementation of future decentralization and other civic education programs in the DRC, as well as implications for the design of future impact evaluations in the field.

B. The *Boîtes à Images* Sessions

In 2007 the Government of the Democratic Republic of Congo (DRC) committed to a process of decentralization, which is to entail the passage of a law on so-called *Entités Territoriales Decentralisées (ETDs)*, creating 26 provinces from the existing 11 and more than 6,000 subprovincial electoral constituencies from the existing 189. The motivations behind decentralization are to bring greater accountability to existing governmental institutions, to reduce levels economic and political inequality across different geographic regions, to enhance the inclusion of broader strata of Congolese civil society, and to provide greater input from communities into decisions related to economic development in their localities (Dizolele 2010). Over the past several years, the process advanced fitfully, and at the moment the ongoing political crisis and tensions between various factions within the DRC government have effectively stalled formal implementation. Nevertheless, at the time of the study (summer 2011) the subject was at least on the agenda of DRC civil society, and officially on the government's agenda as well.

The *VOICE* project initiated by IFES employed a wide range of tools designed to promote greater citizen involvement and understanding of the decentralization process, including information sheets, topical roundtables, and targeted information towards different demographic segments of the Congolese population. The central tool was the *Boîtes à Images* community workshop, in which facilitators used a series of "picture boxes" to illustrate aspects of decentralization and more general political and economic processes to audiences of approximately 100 persons in villages throughout the country. The use of images as a civic education delivery mechanism was designed specifically by IFES as a means to convey messages in the DRC context, where adult literary reaches only 67% in large cities and is likely far less in rural and more remote populations. According to IFES materials provided to *Boîtes* facilitators during their training sessions, the images were designed to be "a tool that triggers dialogue", one that "encourages participants to share what they know, hear, live and understand about decentralization" in "a space where men and women express themselves freely". To this extent, the *Boîtes* sessions were designed to be highly participatory forums where active learning took place, stimulated by the specific images about decentralization that were presented. The *Boîtes* sessions lasted approximately two hours each.

The specific 13 *Boîtes à Images* were organized into more general "modules", corresponding to different aspects of the decentralization process and different aspects of civic education emphasized by the *VOICE* programs. *VOICE* documents describe the modules as follows:

Module 1: "Let's understand our new institutions". Introduce concepts of local governance and importance of participation in local elections and government; explains decentralized institutions and decentralization law (as available); responsibilities of urban and municipal counselors; responsibilities of sector and *chefferie* counselors.

Module 2: "Let's be a part of the new Congo". Introduces the roles of the actors in the process (election commission, politicians, political parties and opposition, judges, civil society); explain rationale for decentralization and benefits of active participation in civic life.

Module 3: "Let's work together for peaceful and fair local elections". Introduce procedures for local elections; explain voting procedures, and encourage peaceful conduct.

The images that formed the basis of this phase of the *VOICE* program primarily emphasized topics in Modules 1 and 2, with less material related to the theme of local elections, as those issues are to be covered more thoroughly in subsequent phases of *VOICE*. Module 1's theme of understanding new DRC political institutions is illustrated by two images reproduced below: Image #2 is entitled "Decentralization in the Democratic Republic of the Congo", and shows the map of the 11 current provinces and a large arrow pointing to the envisioned 26 provinces (including Kinshasa) post-decentralization. Image #8, entitled "Transfer of Resources", depicts the revenue flow envisioned in a decentralized DRC from the local *Entités Territoriales Decentralisées* (*ETDs*) to the central government and back again, with some of the funds to the localities coming from the newly-created *Caisse Nationale de Péréquation*, an institution that will provide funds for local development in order to equalize resources across different geographic regions. Module 2's theme of political participation is exemplified in Image #11 (shown below), "The Role of the Citizen", which depicts ordinary individuals engaging in various acts of political participation, including attending community meetings, submitting a petition to

an elected official, and participating in a peaceful demonstration. Module #3's emphasis on local elections is illustrated in Image #12 below, "The Selection of Leaders", which depicts individuals going to an enrollment center with their required identification documents in order to obtain new voter registration cards. All of the images are accompanied by sample questions to pose to the audience in order to stimulate discussion and learning (e.g. "ask participants to share their experiences in participating in peaceful protests", "ask participants if they know how much revenue their entity mobilizes and what efforts have been made to develop their communities", "ask participants to share lessons learned from previous elections operations"). All 13 images, along with the discussion guidelines and learning objectives for each one, are reproduced in Appendix A of this report.

Image #2: Decentralization in the DRC

Image #8: Transfer of Resources

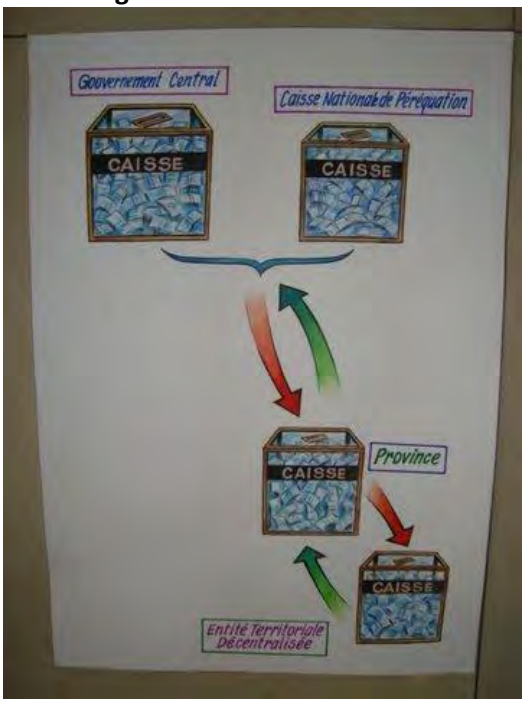


Image #12: The Selection of Leaders

Image #11: The Role of the Citizen

C. Framework for the Evaluation

The *Boîtes à Images* sensitization sessions sought to increase individuals' level of awareness and knowledge of decentralization, political institutions and political actors, increase individuals' awareness of their role in the political process, promote more informed and effective citizen participation, and promote the values and dispositions that would facilitate individuals' contributing to the political and economic development of the Democratic Republic of the

Congo. An assessment of the impact of the sessions, therefore, should focus on these specific orientations and how they may have changed as a result of an individual attending a *Boîtes* session themselves, or as a result of a *Boîtes* session taking place in their village. Further, the assessment should examine the conditions under which the *Boîtes* sessions led to more or less impact on individuals, and the kinds of individuals who responded most strongly to the messages conveyed. The objectives of the study, then, are to determine whether the *Boîtes à Images* sensitization sessions were successful in changing orientations related to decentralization knowledge and awareness, civic skills, democratic values, and political engagement, and if so, under what conditions, and for which kinds of individuals.

Most of the orientations that the *Boîtes* sessions sought to influence — including awareness of politics, skills, support for democratic values, and political participation — are well known to political scientists as precisely those aspects of public opinion that are most important for the development and consolidation of democratic political culture. They have been researched extensively in both established and emerging democratic systems for over fifty years, and have also been examined specifically in the context of assessing the impact of civic education programs in many settings (e.g., Diamond 1999; Almond and Verba 1963; Finkel and Smith 2011; Finkel *et al* 2012). Hence, we have a good deal of knowledge of exactly how these orientations should be conceptualized and measured, and we include indicators used in the past for many of these attitudes, values, and beliefs in the current study. We also develop new questions as appropriate for the DRC context, in particular in gauging citizens' knowledge, awareness and attitudes about the decentralization process, a topic which, to the best of our knowledge, has received very little attention in political attitude research and no attention whatsoever in previous civic education evaluation research.

The study seeks to determine, then, the effects of the *Boîtes à Images* sessions on the following orientations:

- Knowledge about the Decentralization Process in the DRC;
- Knowledge about Political Institutions, Actors and Processes in the DRC;
- Attitudes about Decentralization, including perceived benefits of the process, support for the process, and support for decentralized authority in specific policy areas;
- Political Interest and Political Efficacy, or perceptions of individuals of their political competence, and their ability to participate effectively in political life;
- Political Participation;
- Support for Democratic Processes and Values, such as satisfaction with democracy and political tolerance

The study further seeks to assess the conditions under which the effects of the *Boîtes* sessions were stronger or weaker, focusing specifically on two general sets of factors shown to be relevant in previous evaluations. One set concerns the *quality* and *timing* of the sessions themselves. Previous work has found substantial differences in the effects of civic education exposure, to the extent that workshops or other events are better organized, and led by more competent, knowledgeable, and likeable facilitators (e.g., Finkel and Smith 2011). Moreover, it may be the case that some of the initial effects of civic education exposure may "fade out" over time. We therefore develop measures of the quality (organization, facilitator competence, and the like) of the *Boîtes* session to which individuals were exposed, and test for differential effects

of the sessions based on "session quality", and based on the length of time that may have passed since the *Boîtes* event took place in the individual's village.

The second set of factors that may influence the magnitude of *Boîtes* effects are characteristics related to the individual's political and social resources. Some previous work (e.g., Finkel and Smith 2011) has found greater impact of civic education on those with greater "need" for the information, i.e., individuals with lower levels of education, and lower levels of social and economic resources. We test for these kinds of differential effects in the analyses below as well.

Finally, it is important to consider that the *Boîtes à Images* sessions may have effects that reach well beyond the 80-100 individuals who were directly exposed to the messages conveyed at the event itself. Individuals who did not attend the sessions may nevertheless have discussed decentralization-related topics with individuals who did, thereby becoming exposed to the messages conveyed at the *Boîtes* event indirectly. Recent civic education evaluations have shown the increasing importance of these kinds of "secondary" or "spillover" effects (Finkel and Smith 2011; Fafchamps and Vicente 2010), and there is strong reason to expect the same pattern of findings here as well. We therefore attempt to show the overall effects of the *Boîtes* sessions among individuals living in villages where the events took place, but also devote considerable attention to disentangling the "direct" versus "indirect" effects of the *Boîtes* sessions, on participants and non-participants, respectively.

Chapter II. Study Methodology

The research design of the study has two components: 1) a comparison of changes in decentralization orientations from a pre-event baseline survey and a post-event survey of 1,120 randomly-selected individuals in 8 "treatment" villages in Bandundu province where *Boîtes à Images* events took place, and 480 randomly-selected individuals in 8 comparable "control" villages where *Boîtes à Images* events did not take place; and 2) a randomized "encouragement" experiment, whereby 800 of the treatment village respondents were randomly encouraged to attend the upcoming *Boîtes à Images* event after completing the baseline survey, with their responses compared in several different ways to the "non-encouraged" and "control village" individuals in order to estimate the causal effects of the *Boîtes* events. As we will show, the design allows us to estimate the overall effect of the *Boîtes* sessions at the village-level (component #1), as well as to isolate the causal effect of attending the event itself (component #2). Taken together, the two components also allow us to make some headway in assessing "secondary" effects of the events, such that individuals who were directly exposed to the program may have gone on to discuss the messages of the events with others in their social networks.

In this chapter, we first discuss the research design and its implementation in the field in more detail. We follow with a description of the survey instrument used to assess the respondents' decentralization knowledge, attitudes, and political engagement (the full survey instrument is provided in Appendix B). We conclude with a discussion of the statistical methods used to estimate of the effects of the *Boîtes* events. As we will show, this "pre-test/post-test design with randomized encouragement" allows inferences about the causal effects of the *Boîtes* events on decentralization orientations to be made with greater confidence than in the mainly non-randomized studies used in the field to date. At the same time, the design departs from an

"ideal" experimental design in several important ways, and these limitations need to be taken into account in evaluating the results of the study as well.

A. The Research Design

1. Comparison of Individuals in Treatment and Control Villages

The most basic estimate of *Boîtes à Images* impact comes from a comparison of responses from randomly-selected individuals in villages where *Boîtes* events took place with randomly-selected individuals in comparable villages where the events did not take place. Given the potential for messages about the event to spread to individuals in a given "treatment" village, regardless of whether they personally attended the *Boîtes* event or not, we can estimate an overall impact of the event—including both "direct" effects on attendees and "indirect", "secondary" or "spillover" effects on non-attendees. This is accomplished by comparing individuals in treatment villages with a control group consisting of individuals from comparable villages where *Boîtes* events did not occur. These effects are of critical importance in evaluating the effects of the *Boîtes* events, because the sessions were designed at least in part to influence both individuals who attended the events as well as to stimulate changes among others in the attendees' neighborhoods or social networks.

This component of the study was thus designed to produce two samples of individuals: those residing in 8 villages where a *Boîtes* event was to take place and a comparable sample of individuals residing in 8 villages in nearby (but not too close) *groupements* where *Boîtes* events would not take place. Following the procedures discussed below, the Congolese survey company BERCI interviewed 140 randomly-selected individuals within each of the treatment villages and 60 randomly-selected individuals within control village at two points in time: once within one week before the *Boîtes* event was to take place in the treatment village, and once between one week and one month after the sessions. The basic test of the effects of the *Boîtes* events, then, is a comparison of the *changes* in decentralization orientations registered by the 1120 individuals from the treatment villages with the *changes* in decentralization orientations registered by the 480 individuals from the control villages. We call these the "village-level" effects of the treatment, as we are determining in these analyses the extent to which the *Boîtes* event stimulated changes in decentralization orientations across the entire village, whether the interviewed individuals attended the event or not. These village-level results are shown in section B of chapter III below.

We began by randomly selecting 8 different *groupements* in Bandundu province where we had knowledge that *Boîtes* events were to occur[1], and then asked the NGO that implemented *Boîtes* events in that *groupement* to provide BERCI with a list of villages that were specifically targeted for an event. BERCI selected one village at random from the list to serve as the initial "treatment" village.

BERCI then selected a village to serve as a "control" for each of the selected "treatment" villages. The control villages were to have the following characteristics: they were to be in another *groupement* in the same territory as the treatment village, sufficiently nearby to ensure

[1] Cost considerations prevented us from including a greater number of provinces (our initial design called for including villages in Bandundu, Katanga, South Kivu, and Maniema provinces) and a greater number of villages (our initial design called for 24 treatment and 24 control villages).

general similarities to the treatment village in terms of size, ethnicity, and socio-economic conditions, but sufficiently far away so that there would be little chance of "spillover" effects from the *Boîtes* event taking place in the treatment village. BERCI initially sampled the neighboring *groupements* for each treatment village and then randomly sampled villages from those *groupements* to arrive at the initial list of control villages. If the sampled village was closer than 10 kilometers to the treatment village, a substitute village from the same *groupement* was chosen as a replacement. This produced an initial sampling of 8 treatment and 8 control villages as shown in Table II-1 below.

Table II-1 Initial Sample of Treatment and Control Villages

TERRITOIRE	**GROUPEMENTS IFES**	**Village/IFES (Treatment)**	**POPULATION**
Oshwe	Badjakamba	Popombo	1,540
Idiofa	Ebiala	Ebiala kandolo	1,325
Bagata	Mbaya Salikoko	Mbaya	347
Bulungu	Nkata	Kwanga mulungu PLZ	646
Gungu	Lozo Munene	Lozo Munene 2	1,718
Masi Manimba	Bangalala	Mutulu	343
Kasongo Lunda	Pelende	Tombola	1,087
Inongo	Mbelo	Mpole	1,398
TERRITOIRE	**GROUPEMENTS NON IFES**	**Village/NON IFES (Control)**	**POPULATION**
Oshwe	Etuali Bokala	Beact (camp)	2,159
Idiofa	Bangoli	Maboko	743
Bagata	Falwano	Ngomuna	488
Bulungu	Kikongo	Kikongo	1,381
Gungu	Gatundo	Luvuji 1	165
Masi Manimba	Kalunga Mikinzi	Kingola poste	543
Kasongo Lunda	Kiabamba	Kahungula/Kingete	617
Inongo	Bokote	Bokote/Ilanga	1,073

It became apparent, however, that several of these villages were too remote for *BERCI*'s interviewers to have reasonable access, and it was also apparent that the distances between the "treatment" and its respective "control" village was in some cases so great that travel costs for the surveys would have been prohibitive. With our approval as well as the approval of the local IFES team, three "control" and four "treatment" villages were substituted for the original sampling. We show the final set of villages included in the study in Table II-2 below, with the substituted villages highlighted in boldface. The date that the *Boîtes* event took place in the treatment village is provided as well.

The fact that the resultant sample of treatment and control villages is no longer strictly randomly selected introduces potential biases in the results, to the extent that the substituted villages had characteristics that differentially predisposed individuals towards positive or negative decentralization orientations. That is, with a pure random sample, we can be more confident that the treatment and control villages were matched on important factors that relate to decentralization orientations, and that any differences we observe in the changes on these

orientations over time could be attributable to the *Boîtes* event "treatment".[2] If the sample of treatment villages had, for example, higher levels of education or social integration than the sample of control villages, we could not be certain that differences in decentralization orientations were the result of the *Boîtes* sessions and not due to these other factors.

Table II-2 Final Sample of Treatment and Control Villages

Territoire	Groupements IFES	Village/IFES Treatment	*Boîtes* Date	Groupement Non IFES	Village/Non IFES Control	Estimated Distance
Oshwe	Badjakamba	**Nkole Etat (Pop. 812)**	8/21/11	Etuali Bokala	**Wambia (Pop. 619)**	40km
Idiofa	Ebiala	Ebiala	7/25/11	Bangoli	Maboko	40km
Bagata	Mbaya Salikoko	Mbaya	7/23/11	Falwano	Ngomuna	100km
Bulungu	Nkata	Kwanga	6/22/11	Kikongo	Kikongo	16km
Gungu	Lozo Munene	**Kalumbu (Pop. 435)**	7/16/11	Gatundo	Luvuji 1	50km
Masi Manimba	Bangalala	Mutulu	6/12/11	Kalunga Mikinzi	Kingola Poste	75km
Kasongo Lunda	Pelende	**Munganda (Pop. 522)**	8/7/11	Kiabamba	**Lubombo (Pop. 198)**	27km
Inongo	Mbelo	**Ilungu (Pop. 328)**	9/3/11	Bokote	**Bokoloko (Pop. 90)**	19km

We cannot overcome this potential problem completely, but we do attempt to deal with it in two ways. First, our design explicitly models *change* in decentralization orientations over time between respondents in the treatment and control villages. To the extent that the (non-randomly selected) treatment and control villages have characteristics that would lead individuals to show differences on decentralization orientations at a given point in time, *and to the extent that these characteristics are stable*, these factors will be subtracted out when individuals' orientations are compared from the baseline to the post-test survey. That is, the use of difference scores in the analysis will control for possibly confounding village-level effects, as the procedure any stable village-level characteristic that distinguishes "treatment" and "control" villages (over and above the randomization process) in terms of their average level of decentralization orientations.

Second, as will be discussed in more detail below, we also conduct additional individual-level "matching" of treatment and control group individuals on a set of variables that could be related to decentralization orientations and hence confound the inferences we seek to make about the effects of *Boîtes sessions*. This procedure resulted in a treatment and control sample of individuals who were effectively "balanced" in terms of age, sex, education, political interest and exposure to mass media *before* the treatment (*Boîtes* event) was introduced in the treatment villages. This increases our confidence that whatever deviations from pure random

[2] It is also the case that the *Boîtes*/Voice program itself was not randomly implemented at the village level. This introduces the possibility that the village chosen by IFES for "treatment" were potentially more receptive to the messages in the *Boîtes* session than villages not chosen for treatment. Some of the procedures we implement here can help control these potential biases but whenever treatment placement is non-random they cannot be eliminated altogether (see Khandker et al. 2010).

sampling the village-level substitutions may have produced, the treatment and control individuals in the analysis were nevertheless effectively identical on variables known to be related to decentralization orientations, attitudes, and engagement.

2. The Randomized "Encouragement" Component

a. The Logic of the Design

We supplement treatment and control comparisons at the village level with an additional component designed to assess the specific effects of attending a *Boîtes à Images* sensitization session. This is an important aspect of the design for several reasons. First, it may be the case that no village-level effects will be detectable following the procedures described above; to the extent that the proportion of individuals who attend the events in a particular village is low, a random sample of 140 villagers may not produce sufficient numbers of attendees to show significant differences from the control villages, even if significant effects of exposure to the treatment exist. Second, we want to determine what the effect of direct exposure to the treatment actually is, aside from any spillover effects resulting from post-exposure discussion within individual social networks.[3] To determine the "direct effects" of exposure to the *Boîtes* events, we therefore need to compare those individuals who attended the *Boîtes* events with individuals who did not attend.

The most straightforward way to make these comparisons is simply to ask individuals in the treatment villages whether or not they attended the *Boîtes* event that had recently taken place during the post-test survey. There are two significant difficulties in relying solely on this approach (though of course we did collect this information and it does figure prominently in the subsequent analysis). First, as noted above, we typically have little idea *a priori* how many individuals will attend a given civic education event. If few individuals in a village attend, relative to the number of individuals interviewed, we will have a very small sample of "treated" individuals to analyze, and concomitant difficulties in demonstrating the direct effects of the treatment. And since the proportion of attendees is not known to us in advance, we have no way of knowing how large a random sample of a village would need to be in order to produce sufficient numbers of attendees.[4] This presents a practical difficulty in estimating the effect of *Boîtes* attendance.

A more fundamental difficulty, however, is that individuals choose to attend the *Boîtes* events (as in virtually all civic education programs), and this "self-selection into treatment" introduces serious potential biases in the assessment of treatment effects. Self-selection into treatment means that it is impossible, even after introducing multiple control variables within a statistical matching or regression context, to rule out the possibility that unobserved factors leading individuals to attend the *Boîtes* events also produced higher levels of knowledge, engagement, or other decentralization orientations. "Selection on unobservables" such as motivation or

[3] It may even be the case that spillover effects are negative, which again could produce small or negative village-level treatment effects in the analyses above, even if there were significant direct effects on individual attendees.

[4] Finkel and Smith (2011) "solved" this problem by interviewing randomly-selected attendees at Kenya civic education workshops at the event sites before the workshop began. This procedure guaranteed a sufficient number of attendees, but potential biases stemming from the self-selected nature of workshop attendees nevertheless remained.

personality is likely in the case of most civic education programs, as those individuals who are "predisposed" towards democracy or who are already "trending" towards democratic engagement are likely to be those individuals who voluntarily attend community workshops or other activities associated with democracy. As a result, a simple comparison of "attendees" versus "non-attendees" is likely to produce a biased estimate of program impact to the extent that these "unobservables" confound the estimates of interest.

Ideally, we would be able to *randomly assign individuals to the treatment*, so that we would select a random sample of villagers to comprise the participants in a given *Boîtes* event, and compare their responses to a random sample of villagers who did not attend (along with a random sample of individuals in control villages as described above). Such a design was impossible to implement in this case, because, as with virtually all civic education programs outside of the school context, we can neither randomly assign individuals to the treatment, nor can we mandate compliance with any manipulation that would be imposed. That is, we cannot compel individuals to attend workshops, and in the usual case we cannot deny access to the event to another person in a given village who is part of our survey sample.

To overcome these difficulties, we implemented a variation of the "encouragement design" described in Duflo, Glennerster and Kremer (2007), in which some randomly selected individuals are encouraged through a baseline interview process to experience a given kind of treatment while others are not given such encouragement. The encouragement design works by exogenously increasing the *probability* of attending some event as a result of the *randomly-assigned invitation* to take the treatment. If the encouragement is successful, we can then compare the overall attitudes, values, or behaviors among individuals in the "encouraged to treatment" group with both the "not encouraged" and the control village groups in various ways to obtain estimates of program impact that preserve the core advantage of random manipulation in making valid causal inferences. As we will show below in the section on statistical modeling, we make initial comparisons of these groups in what is referred to as "intent to treat" effects, and subsequent analyses will go further in using the "encouraged to treatment" variable as what is known as an "instrumental variable" that proxies for self-reported treatment exposure. The randomization process ensures that this proxy variable is unrelated to all factors —including unobservables— that are correlated with both treatment exposure and decentralization orientations. Thus, provided that the encouragement works as intended, the design allows for the estimation of treatment effects controlling for the possible confounding influences of self-selection.[5]

Figure II-1 below summarizes both components of the research design. We interviewed 140 individuals in each of the 8 treatment villages, and make initial comparisons between the 1,120 total treatment village responses and the 480 total respondents interviewed in the 8 control villages (Groups A+B versus Group C). Within each treatment village, moreover, we divide the respondents randomly between an "encouraged" group (Group A) and a "not encouraged" group (Group B), and we use comparisons between all the groups, along with information about their exposure to the *Boîtes* event, in order to arrive at estimates of the direct effect of the treatment.

[5] More technically, the design identifies the average treatment effect among "compliers" with the manipulation, that is, among those individuals who were induced to experience the treatment through the invitation but who otherwise would not have attended.

We note two other advantages of the design. First, it may be possible to compare the village-level effects of the *Boîtes* event with the specific effect for attendees to arrive at an estimate of "secondary" or "spillover" effects of the sessions. These estimates may be further augmented through comparison of the non-encouraged portion of the treatment village sample with the control village sample (i.e., Groups B versus C) and comparisons of the non-attendees in the treatment village with individuals similar on background characteristics from the control villages (see chapter III, section D below).

Figure II-1 Summary of Research Design

	Respondents Per Village	**Total Respondents**
Treatment Villages (8 Total)	140	1,120
Encouraged to Attend *Boîte* Event **(Group A)**	100	800
Not Encouraged to Attend *Boîte* Event **(Group B)**	40	320
Control Villages (8 Total) **(Group C)**	60	480
Totals: Baseline Survey	200	1,600
Totals: Post-Test Survey	200	1,600

Second, the encouragement design is also beneficial in overcoming one of the problems in assessing the effects of *Boîtes* attendance discussed above, i.e., the potential lack of attendees in the analysis sample. By introducing a random encouragement to attend the *Boîtes* event in the pre-test (baseline) survey, we also increase the likelihood that a sufficient number of individuals among those sampled were actually in attendance at the event.

B. Survey Instrument and Variables Used in the Analysis

The survey instrument included questions relating to the main themes of the *Boîtes à Images* program: decentralization, democracy, and political engagement. Key questions relate to individuals' *knowledge, attitudes,* and *opinions* about the decentralization process and policies in the DRC, and we categorize these items under the general rubric of *Decentralization Knowledge and Attitudes.* Other questions relate to individuals' general *knowledge* about politics, *preferences* about democracy, and *engagement* with politics. We categorize these items under the rubric of *Political Knowledge, Political Attitudes, and Engagement.* The survey instrument also asked *Basic Demographic* information. Finally, in the baseline survey a random sample of individuals in treatment villages were provided the "encouragement to attend" the upcoming *Boîtes* event. In the post-*Boîtes* survey, all individuals were asked if they had attended the session as well, with those who answered "yes" being asked a further series of questions related to their experiences and satisfaction with various aspects of the event, as will be discussed below. Many of the survey questions were drawn from previous civic education

evaluations; others represent new items developed for the DRC and decentralization process, about which there is little survey-based research from which to draw.

The questions were vetted through the IFES office in Washington and in Kinshasa, and were pre-tested with a small sample of 100 individuals by BERCI in December 2010 (and subsequently modified as appropriate). The questionnaire was developed originally in English by Steven Finkel; it was translated into French by BERCI personnel and verified by French speaker Rola Adbul-Latif in the IFES Washington office. The final questionnaire was translated into two local languages, Lingala and Kikongo, for administration in the field.

Decentralization Knowledge and Attitudes

Respondents were asked a series of questions regarding their general and specific knowledge of the decentralization process in the DRC, its positive and negative aspects, their general support for decentralization, and their satisfaction with the pace of the process. Respondents were also asked their views on whether specific policy areas should be the responsibility of the central or local governments.

Decentralization Knowledge. Respondents were asked two general and four specific questions about knowledge of the decentralization process in the DRC. The two general questions were:

1. Do the decentralization laws give more power to the central government in Kinshasa, less power to the central government in Kinshasa, or does it not change the amount of power the central government in Kinshasa has?
2. Do the decentralization laws give more power to the local and Provincial governments, give less power to the local and Provincial governments, or does it not change the amount of power the local and Provincial governments have?

The four specific questions were:

1. Do you happen to know how many provinces there are to be in the country, including the city of Kinshasa, once the laws about decentralization are passed? Is the number less than 10, between 10 and 20, between 20 and 30, or more than 30? [CORRECT ANSWER: 26]
2. The laws about decentralization have created smaller units within each province that are called *Entités Territoriales Decentralisées* or *ETD*. One of these entities is *la Chefferie*. Do you happen to know who makes the laws for *la Chefferie*? **[**CORRECT ANSWER: *le Conseil de Chefferie*]
3. Decentralization has also changed the way that provinces and local governments get money to provide services for the people. Some of the money is supposed to come from local taxes, while some is supposed to come directly from the central government in Kinshasa. Think about a scale that runs from 0 to 100. If 0 means that *none of the money for local governments comes from the central government in Kinshasa,* and 100 means than *all of the money for local governments comes from Kinshasa*, could you tell me what share of the money for local governments you think is supposed to come from Kinshasa? [CORRECT ANSWER: 40%]
4. Have you ever heard of something called the "*Caisse Nationale de Péréquation*?" If yes, "is the *Caisse Nationale de Péréquation* something that is supposed to give money for people who don't have enough food to eat", "give money to village chiefs to settle land disputes and other problems in the villages," "give money to local governments to support development projects" [CORRECT ANSWER], or "give money to returning combatants from the war to help them start new lives"

We summed respondents' correct answers to create a *Decentralization Knowledge* scale ranging from 0 to 6.

Perceived Knowledge of Decentralization. We asked respondents the extent to which they felt informed about the decentralization process. The question was coded as "1" for "not informed at all" to "4" for "very informed."

Positive and Negative Aspects of Decentralization. The survey instrument included questions on the extent to which respondents agreed or disagreed with the following statements: (1) Decentralization will be good for the country because the local governments will be more able than the Kinshasa Government to find solutions to the population; (2) Decentralization will be good for the country because the DRC is too big to be controlled by the central power only; (3) Decentralization is not a good thing for the country because it will generate more conflict than economic and political resources; (4) Decentralization will be good for the country because it will give more control to ordinary citizens on economic development; and (5) Decentralization is not a good thing for the country because it will bring more corruption by authorities than before. Responses were scored as "1" for "strongly disagree" to "4" for "strongly agree." We created two scales referring to the positive aspects of decentralization (items 1, 2, and 5) and the negative aspects of decentralization (items 3 and 4).

General Support for Decentralization. We measured respondents' support for decentralization by asking respondents the extent to which they agreed or disagreed with the following assertion: "In general, I support the decentralization process in the DRC." Responses were scored as "1" for "strongly disagree" to "4" for "strongly agree."

Pace of Decentralization. We asked respondents whether they thought decentralization process was going too fast, too slow, or at the appropriate pace. Responses were coded as "1" for "too fast," "2" for "about the right pace," and "3" for "too slow."

Policy responsibilities. Respondents were asked about their opinions on whether the central government in Kinshasa or local governments should be responsible for different policy areas including: (1) roads and transportation; (2) schools and education policy; (3) economic policy and taxation; (4) the police; and (5) the military. For each item, responses were scored on a four-point scale, with "1" for "completely the central government," "2" for "mainly the central government," "3" for "mainly the local governments," and "4" for "completely the local governments." We collapsed the scales (1 & 2 = central government and 3 & 4 = local governments) coded them as "0" for "central government" and "1" for "local government." We combined the responses for police and military authority into a single measure.

Political Knowledge, Political Attitudes, and Engagement

We asked a series of questions concerning the respondent's general knowledge of how the political system of the DRC works, their perceived knowledge of the constitution, their support for democracy as a form of government and their level of satisfaction with it, and their support for political values associated with democratic governance. Respondents were also asked about their perceived ability to participate in politics, their interest in political matters, how often they discuss politics with others, and the extent to which they participate in political activities.

General Political Knowledge. Respondents were asked four questions about knowledge of political matters in the DRC: (1) whether parents have to pay for their children to go to elementary school [CORRECT ANSWER: NO]; (2) who appoints members of the Supreme Court [CORRECT ANSWER: THE PRESIDENT]; (3) which political party got the most votes in the 2006 election [CORRECT ANSWER: PPRD]; and (4) whether amendments to the constitution of the Democratic Republic of Congo made by a *declaration of the President*, by a *simple majority of the national assembly*, by a *two-thirds majority of the national assembly*, or by *a majority vote*

by the people. [CORRECT ANSWER: TWO-THIRDS MAJORITY]. We summed respondents' correct answers to create a *Political Knowledge* scale ranging from 0 to 4.
Perceived Knowledge of the Constitution. We asked respondents the extent to which they felt informed about the content of the Constitution of the DRC. The question was coded as "1" for "not informed at all," "2" for "somewhat informed," and "3" for "very informed."
Democracy is Best. We asked respondents, "Sometimes democracy does not work. When this happens, some people say that we need a strong leader that does not have to bother with elections. Others say that even when things do not work, democracy is always the best option. Can you tell us which statement is closest to your view?" Responses were coded as "0" for those who say that the country needs a strong leader and "1" for those who say that democracy is always best.
Satisfaction with Democracy. We asked respondents whether they were satisfied with the way democracy works in the DRC. Responses scored as "1" for "not at all satisfied," "2" for "somewhat satisfied," and "3" for "very satisfied."
Political Tolerance. We included a battery of questions testing respondents' willingness to extend basic political rights to "people whose ideas are considered bad and dangerous." Respondents were asked whether (1) a person who does not like their tribe should be allowed to participate in political activities of the village, and (2) a person who would abolish elections and install a military dictatorship should be allowed to participate in elections. The questions were scored from "1" for "strongly disagree" to "4" for "strongly agree." We averaged the two items to create a *Political Tolerance* scale.
Political Efficacy. We probed respondents about their perceived influence in politics, or political efficacy, by asking individuals whether they agreed with the following statements: (1) "I feel very prepared to participate in the political life of my country," and (2) "The new decentralization law is too complicated to understand for people like me." Responses were scored as "1" for "strongly disagree" to "4" for "strongly agree." We averaged respondents' answers to create a *Political Efficacy* scale ranging from 1 to 4.
Political Interest. Respondents were asked about their interest in political matters in the DRC. Responses were scored as "1" for "not at all interested," to "4" for "very interested."
Political Discussion. We asked respondents whether they have discussed political matters with friends, family, or work colleagues in the past year and how many times they had done it. Responses were coded as "0" for "not done," "1" for "once," and "2" for "several times."
Political Participation. The survey instrument included questions on whether the respondent had done any of the following activities in the past year: (1) participated in a collective effort to solve the problems of the neighborhood or community; (2) filed a complaint with a government agency or civil society organization about an injustice or violation of their rights; and (3) taken part in a protest, march, or demonstration on some national or local issue. For each item, responses were scored on a three-point continuum, with "0" for "not done," "1" for "once," and "2" for "several times." We created a scale of political participation by summing individuals' positive responses to the questions ("once" and "several times").

Demographic Variables
Respondents were asked basic demographic questions such as age, sex, and level of education, and also whether anyone in their household had been victims of violence during the recent conflict in the DRC. These variables were measured as follows:
Age. We asked respondents about their age in years. Responses were collapsed and coded in the following categories: "1" for "18 to 24," "2" for "25 to 34," "3" for "35 to 49," and "4" for "50 to 86."

Sex. Responses were coded as "0" for female and "1" for male.
Education. Respondents were asked about the highest level of formal education attained. We coded responses as "1" for "no schooling and cannot read and write," "2" for "never attended school but can read and write," "3" for "some elementary schooling," "4" for "finished elementary schooling," "5" for "some secondary classes," "6" for "secondary school finished," "7" for "some college," and "8" for "college graduate."
Violence Experienced. We included a question that asked whether respondents had lost a member of their immediate family during the recent armed conflict in the DRC. Responses were scored as "0" for "no" and "1" for "yes."

Encouragement to Attend the Boîtes Session. Within each treatment village, BERCI selected 100 individuals at random to form an "encouraged" sample, and 40 individuals at random to form a "not-encouraged" sample. Individuals in the "encouraged" group were told the following:

> *Thank you very much. We've finished the interview, but before we leave, I'd like to tell you about an event that is going to take place this _DATE OF BOITE EVENT_ at _PLACE OF BOITE EVENT_. This gathering will be an animated civic discussion with images related to the process of decentralization that is now going on in the country. It is organized by _NGO NAME_, an organization that is not affiliated with the government nor with any political party. The event will take place at the following address: _ADDRESS OF BOITE EVENT_.*
>
> *There won't be room for everyone to come, so our company has asked us to invite some of the people we are talking to participate in the event. Do you think it would be possible that you would participate? (IF YES): After the event takes place, if you give them your name, the facilitators will reimburse you any of your travel expenses.*[6]

Attendance at Boîtes Session. In the post- *Boîtes* survey, all respondents were asked whether they had attended the *Boîtes* session that had recently taken place in the treatment village. The wording was as follows:

> *Last week (month), there was a meeting in _VILLAGE OF BOITE EVENT_ sponsored by _NGO NAME_ that talked about issues related to decentralization in the DRC. Did you participate in this event?*

We attempted to verify the participation of each individual in our survey who claimed to have attended a particular *Boîtes* event, but this proved virtually impossible given the incomplete lists available, transcriptions of individuals' names that proved difficult to read, and other problems in the field. Nevertheless, we were able to make some headway in this regard by asking all individuals who claimed to have had attended the event to answer the following question:

> *"Could you please tell how the Boîtes à Images civic education event was conducted? Was it through: an oral presentation by a leader, a presentation and comments about images, or through presentations of films and videos?*

We treated individuals who did not answer "a presentation and comments about images" as having a strong likelihood of *not* being in attendance despite their initial claim. This resulted in a

[6] Importantly, *all* individuals who attended the event were given the equivalent of $5 to offset travel expenses, whether or not they were encouraged to attend or not. This prevented any bias resulting from resentment of non-encouraged attendees who may nevertheless have been in our analysis sample.

77% attendance rate among individuals in the treatment sample, compared with the 87% attendance rate as measured by the initial claims of the respondents.

Session Quality. We included a battery of questions inquiring about individuals' impression of the quality of the *Boîtes* session they attended. Respondents were asked the extent to which they were satisfied with a) the organization of the session; b) the information they learned at the session; and c) the competence of the facilitators who led the session. Each question was coded from "1" for "not at all satisfied" to "4" for "very satisfied." Were averaged the three items to create a *Session Quality* scale.

C. Field Work

Baseline interviews were begun by BERCI in the period between June 8, 2011 (Bulungu *Territorie*) and July 23, 2011 (Kusango Lunda *Territorie*). In each territory, the baseline survey was conducted within the week prior to the *Boîtes à Images* session taking place in the treatment village. Interviewers followed standard random route household interviewing procedures (described in pages 8-9 of the technical appendix from BERCI, found in Appendix C of this report). Detailed information about the respondent and how s/he could be recontacted in a future follow-up survey was collected. The procedures used to verify that the respondents were identical in the baseline and post-*Boîtes* interviews are also found in Appendix C of the report. Following the *Boîtes* session, interviewers attempted to reinterview all respondents from the baseline wave. This proved highly successful, as BERCI achieved a remarkable 98% reinterview rate. There were no differences in panel attrition rate between treatment and control villages. Post-*Boîtes* interviews were conducted between July 8, 2011 and September 4, 2011; this period represented a time of between 1 and 26 days following the *Boîtes* session in the treatment villages. The overall time between pre-event and post-event interviews ranged from 3 to 39 days.

D. Statistical Analyses

1. Estimating Village-Level Effects of the *Boîtes* Sessions

We estimate the effects of the *Boîtes* events at the village level through comparisons of the pre- and post-event levels of each dependent variable among individuals in the "treatment" and "control" village samples. Using standard notation so that subscript *i* refers to an individual, subscript *t* refers to the time or "wave" of observation, and subscript *j* refers to a village, we can express, for example, the individual's knowledge about the decentralization process in the DRC at a given point in time as:

(1) $y_{ijt} = \beta_{0t} + \beta_1 D_{it} + \beta_2 I_i + \beta_3 J_j + \varepsilon_{ijt}$

where D_{it} represents prior exposure to the *Boîtes* event, (coded as "0" for all respondents at time *t=1* and for control village respondents at time *t=2*, and coded as "1" for individuals in treatment villages only at time *t=2*), I_i represents all individual-level characteristics that are unique to individual *i* and that are stable over time, and J_j represents all stable village-level characteristics that are unique to the given village *j*, the β represent regression coefficients linking the independent variables to the outcome variable *y*, and ε is an error term. The equation models the individual's score on *Decentralization Knowledge* (*y*) at a given point in

time as a function of an overall average score for all individuals at that time β_{0t} plus some effect β_1 from having been exposed to the *Boîtes* event, along with effects from stable factors unique to each individual – either observed factors such as age or education level, or unobserved factors such as "personality" – and stable factors (e.g. observed socio-economic level or unobserved level of "social capital") unique to the village.[7] This equation can be expressed for each individual in the treatment and control villages at both points in time, and an expression for the *difference* or change in decentralization knowledge (and other dependent variables) can be obtained by subtracting the equation at time 1 (pre-*Boîtes*) from the equation at time 2 (post-*Boîtes*). This results in the following basic equation for estimating the effect of the treatment at the village level:

(2)

$$y_{ij1} = \beta_{01} + \beta_1 D_{i1} + \beta_2 I_i + \beta_3 J_j + \varepsilon_{ij1}$$
$$y_{ij2} = \beta_{02} + \beta_1 D_{i2} + \beta_2 I_i + \beta_3 J_j + \varepsilon_{ij2}$$
$$\Delta y_{ij} = \Delta\beta_0 + \beta_1 \Delta D_i + \beta_2 \Delta I_i + \beta_3 \Delta I_i + \Delta\varepsilon_{ij}$$

which, since the individual and village level I and J terms are assumed to be stable, reduces to the following basic estimation equation for assessing the effects of the *Boîtes* event:

(3) $\Delta y_{ij} = \Delta\beta_0 + \beta_1 \Delta D_i + \Delta\varepsilon_{ij}$

Alternatively, since ΔD_i equals 0 for all individuals in the control villages and 1 for all individuals in treatment villages, the equation can be expressed as modeling the "difference in the average difference" in the dependent variable for the treatment and control villages:

(4) $\Delta y_{ij} = \Delta\beta_0 + \beta_1 TREATMENT_i + \Delta\varepsilon_{ij}$

where $\Delta\beta_0$ then represents the average change over time in *y* for control villages, and β_1 represents any additional change in *y* for individuals in treatment villages. If the β_1 coefficient is statistically significant, this indicates that the change in decentralization knowledge (or other dependent variables) in treatment villages is statistically different from the change in control villages. And because both individual and village level "fixed effects" *I* and *J* have been included and then differenced out of consideration, this model represents a very powerful means of estimating causal effects of the treatment, independent of unobserved or unmeasured factors at the individual or village levels that may also influence decentralization knowledge, provided that those factors can be assumed to be stable over time.

All of the models are estimated using basic ordinary least squares (OLS) regression methods and so-called "robust clustered" standard errors for purposes of statistical inference. Robust clustered standard errors take into account the fact that observations from individuals within a given village are likely to be related to one another to a significantly greater extent than are observations from individuals in different villages, thus violating the OLS assumption that sampled observations are statistically independent. These robust clustered standard errors

[7] The unobserved components of the *I* and *J* terms are also commonly referred to as individual and village level "fixed effects".

provide a more conservative test of statistical significance, as the tests are effectively based on the much smaller number of independent village-level observations (16), compared to the total number of individual observations (1,600).[8]

The model in equations (3) and (4) easily can be extended to include additional control variables, for example, educational attainment or age, and we report the results of models that control for these factors, along with sex, political interest, and media attentiveness, in subsequent analyses as well. These models, in effect, relax the assumption in (2) above that the regression coefficient linking these variables to *y* is the same in waves 1 and 2, and thus test for whether the *Boîtes* session affects changes in knowledge, over and above differential changes over time shown by individuals with different levels of education (or sex or political interest).

The main limitations of these analyses stem from the lack of complete randomization of treatment and control villages in the study, as noted above. This means that the treatment and control villages may differ on important factors related to decentralization orientations before the *Boîtes* treatment, and these pre-existing differences, and *not* the treatment itself may be responsible for differential changes observed between the treatment and control villages over time. Some of this problem is taken into account through the differencing process shown in (2), and some of the problem is further taken into account by including additional control variables in the estimation of equation (4). Nevertheless, we implement an additional method known as "coarsened exact matching" (CEM) to further account for possible pre-existing differences between individuals in treatment and control villages (see Finkel, Horowitz, and Rojo-Mendoza (2012) for a recent application in civic education evaluation).

Table II-3 Covariate Imbalance, Pre- and Post-CEM balancing

	Pre-matching Means		**Post-matching Means**		
Variable	Control	Treatment	Control	Treatment	Scale
Sex	0.55	0.53	0.56	0.53	0 = female 1 = male
Education	3.32	3.28	3.27	3.28	0 = no schooling 7 = university graduate
Age	**37.74**	**35.78**	35.80	35.77	18-86 years old
Media Attentiveness	**0.95**	**1.04**	1.02	1.04	0 = never 3 = everyday
Political Interest	**2.84**	**3.12**	3.11	3.12	1 = very little interest 4 = great deal of interest
Discuss Politics	0.82	0.82	0.86	0.81	0 = never 1 = once 2 = several times
N of Cases	471	1,101	470	1,084	

Note: Boldfaced comparisons indicate significant differences at .05 level between treatment and control villages.

[8] Clustered standard errors may be biased when the number of groups (villages) is relatively small, as in the current study. As a further robustness check, we use a more conservative procedure known as "randomization inference" in subsequent models whenever our initial estimates of equation (4) show a significant effect of the *Boîtes* treatment. (See Rosenbaum 2002).

The CEM matching procedure produces different "bins" or strata consisting of the combinations of characteristics in the specified variables (e.g., women with high education, medium interest, young age), and then balances the treatment and control groups as completely as possible by eliminating treatment group cases that have no corresponding or "matched" control group member in their "bins", and eliminating control group cases that have no corresponding or "matched" treatment group member in their "bins" as well. In our case, this procedure eliminated only 18 individuals, indicating that there was very strong initial balance between the treatment and control villagers on the "coarsened" variables. Table II-3 above shows the degree to which individuals in these groups were balanced before and after the CEM matching procedure.

As the table shows, whatever differences existed between the treatment and control village samples on age, media attentiveness, and political interest in the baseline surveys were eliminated through the CEM matching and balancing procedures. In subsequent analyses, we match further on the pre-treatment level for each dependent variable, such that the estimation sample is *fully balanced* on the pre-*Boîtes* outcomes between the treatment and control villages. This means that we are estimating the effect of the *Boîtes* treatment on individuals in the treatment and control villages who are similar on important demographic and political characteristics *and* who have equivalent initial values of decentralization knowledge and the other outcomes of interest in the study. All of these procedures ensure that we are estimating the effect of the *Boîtes* events while controlling for as many pre-existing differences between the treatment and control villages as possible, given the lack of full randomization of treatment at the village level in the study design.

2. Estimating the Effects of Attending *Boîtes* Sessions

Following the logic of the exposition above, we also seek to estimate the effect of the *Boîtes* events on the specific individuals who attended the sessions in the treatment villages. We exploit the features of the "encouragement" design, whereby 100 randomly-selected treatment village individuals were invited to the *Boîtes* session in the baseline wave of data collection, while 40 randomly-selected treatment village individuals, along with the 60 randomly-selected control village respondents, were not told about the *Boîtes* event. The encouragement is designed to (randomly) increase the encouraged individuals' probability of attending the *Boîtes* session, and this "exogenously-induced" increase can be used in several ways to estimate the impact of attendance. The fact that the increase *is* exogenously or randomly-induced means that attendance, at least for some segment of the encouraged group, can be viewed as unrelated to any other factor — either observable or unobservable — that may be correlated with both attendance at the *Boîtes* sessions and decentralization orientations and other dependent variables. We may then use this information to estimate of the effects of the *Boîtes* sessions, over and above the potential biases stemming from the self-selection of individuals into the treatment. In our case, the randomization of encouragement worked nearly perfectly in the field, as there were virtually no differences between "encouraged" and "non-encouraged" individuals from the treatment villages on demographic, political, or decentralization-related variables. We thus produced groups that were statistically identical aside from their being encouraged to attend the upcoming *Boîtes* event.

We begin by estimating the "difference in differences", i.e., the difference in each outcome from the baseline interview to the post-event interview, between individuals who were encouraged

to attend the *Boîtes* event and individuals who were not encouraged to attend. This so-called *"intent to treat"* effect estimates the impact of *Boîtes* attendance on the randomly-selected treatment group without considering possible "non-compliance" with the manipulation. As non-compliance is likely to be related to factors associated with the outcomes in the study (e.g., political interest, motivation, overall knowledge about politics), the analysis of the randomized encouraged group versus the randomized non-encouraged group provides a "pure" estimate of the potential impact of the treatment. The *"intent to treat"* estimate results from a straightforward substitution of an "Encouragement to Attend" (E_{it}) indicator variable for the treatment variable *D* in equation (2) above, as:

$$
\begin{aligned}
y_{ij1} &= \beta_{01} + \beta_{11}E_i + \beta_2 I_i + \beta_3 J_j + \varepsilon_{ij1} \\
y_{ij2} &= \beta_{02} + \beta_{12}E_i + \beta_2 I_i + \beta_3 J_j + \varepsilon_{ij2} \\
\Delta y_{ij} &= \Delta\beta_0 + \beta_{12}E_i + \beta_2 \Delta I_i + \beta_3 \Delta J_i + \Delta\varepsilon_{ij} \\
\Delta y_{ij} &= \Delta\beta_0 + \beta_{12}E_i + \Delta\varepsilon_{ij}
\end{aligned}
\tag{5}
$$

where E_i represents the randomized encouragement to attend the *Boîtes* session and with all other terms as above.[9] Since the *E* term is randomly determined, and hence is unrelated to all factors in the unobserved error term ε (or Δε) that may also be related to the dependent variables, β_{12} provides an estimate of the *Boîtes* treatment that is based on the logic of randomized assignment.[10]

At the same time, as the amount of non-compliance increases, i.e., the more that encouraged individuals do not attend and non-encouraged individuals do attend, the estimated "intent to treat" effects will be correspondingly weaker. In our case, we observed significant amounts of non-compliance: 83% of the encouraged sample was coded as having attended the event, while a full 62% of the non-encouraged sample (within treatment villages) attended the events as well. This means that the randomized manipulation to attend was successful in influencing the likelihood of participation in the event, but the difference in self-reported attendance between the encouraged and non-encouraged groups in the treatment villages was only about 20%. This means that the encouragement manipulation has less statistical power than it would have had the "compliance" with the manipulation among non-encouraged individuals been higher; it also means that it is likely that the "*intent to treat*" estimate will underestimate the "true" causal effect of the treatment, given dilution from non-compliance.[11]

---

[9] Note that the *E* variable does not drop out through the differencing process because its causal effect on the outcome variable is assumed to change over time. In the baseline wave of observation *E* is assumed to have no effect on the outcome variable, i.e., being encouraged or not to attend the *Boîtes* event is assumed to have no direct influence on (pre-*Boîtes* levels of) decentralization orientations. This means that β_{11} equals 0 in (5a), while β_{12} will be greater than 0 in (5b), to the extent that encouragement does raise the probability of attending the *Boîtes* event and to the extent that the events have effects on individuals' decentralization orientations.

[10] As in the village-level analyses, we also control for other potential confounding variables, including the lagged value of the dependent variable, by including them in the regression model and by balancing them at the start of the analysis through the CEM procedure described above. In the "intent to treat" models, we balance the encouraged and not-encouraged groups on the relevant covariates.

[11] To the extent that non-encouraged individuals may discuss the *Boîtes* event with individuals who do attend, these potential "spillover" effects will also weaken the "intent to treat" estimate in (5).

We can obtain a second estimate of the effects of *Boîtes* attendance by comparing the responses of individuals who report that they attended the session (in either the encouraged or non-encouraged groups) with all individuals who did not attend. We may call this the *"self-reported attendance"* model, which takes into account non-compliance with the encouragement manipulation above, but ignores the advantages that result from the randomization process. This model is identical in form to the previous village level and "intent to treat" models, but with an indicator variable "ATTENDED" (A_i) used as the key independent variable. The "self-reported attendance" model is similar in design to previous observational evaluations of the effects of civic education, which compared self-reported attendees and non-attendees in estimating causal effects of the programs (e.g., Finkel 2003; Finkel *et al* 2012).[12] One major advantage of this design is that we have both baseline and post-event data for all respondents, so we are able to exploit the longitudinal nature of the data and difference out both individual and village level fixed effects as noted above (see also Finkel and Smith 2011). Still, there are potential self-selection biases in the "self-reported attendance" model, to the extent that attendance at the event is associated with time-specific unobserved factors, or unobserved factors that also are associated with *changes* (as opposed to simple levels) in the dependent variables over time. For example, *Boîtes* attendees may have been trending upwards in their decentralization knowledge already, and this increased knowledge may have led them to attend the event and also show subsequent changes on knowledge *that would have occurred in any case*, with or without having attended the *Boîtes* session. We thus report the results of the self-reported attendance model but rely on it cautiously as an accurate estimate of the causal effects of exposure to the *Boîtes* events.

The final estimate of the impact of *Boîtes* attendance attempts to overcome the self-selection biases on the *self-reported attendance* model by exploiting the randomized nature of the initial encouragement design. In the self-reported attendance model,

(6) $$\Delta y_i = \Delta\beta_0 + \beta_1 A_i + \Delta\varepsilon_i$$

the problem stems from the potential relationship between attending the event (A_i) and unobservables captured in the error term ε_i, which may bias the ordinary least squares estimation of the effects of A_i on Δy_i. To overcome this potential problem, we need an "outside" exogenous variable, called an "instrumental variable", which is related to attendance (A_i) at the event but unrelated to any of the unobservables or other components of the error term. Because the study design implemented here was successful, the *randomized encouragement variable E_i satisfies these criteria and can serve as a valid "instrument" in assessing causal effects.* Since E_i did increase the probability of attending the *Boîtes* event (as noted above), it satisfies the first criteria. And since individuals in treatment villages were randomly assigned to receive the encouragement, E_i is therefore statistically independent of *any* other variable, either observed covariates or unobserved factors in ε_i. It therefore satisfies the second criteria for serving as an appropriate instrumental variable as well.

The estimation for this *"instrumental variable"* (IV) model involves a two-stage procedure. In the first stage, we regress A_i on the instrumental variable E_i and all control variables Z_k that may

[12] We also note that there are fewer "non-attendees" in the sample than we would like (N=252, or 23% of the treatment village sample), but as mentioned above it was not possible to estimate accurately *a priori* the proportions of likely attendees.

be related to attendance at the *Boîtes* events and generate a "predicted" attendance variable A^*_i:

$$(7) \quad \begin{aligned} A_i &= \beta_0 + \beta_1 E_i + \beta_k Z_k + u_i \\ A_i^* &= \beta_0 + \beta_1 E_i + \beta_k Z_k \end{aligned}$$

Because A_i is predicted by exogenous or random variables that are unrelated to the outcome equation's error term ε_i, then A^*_I is also unrelated to those error terms. In the second stage, we use A^*_I as the "proxy" for A_i in the self-reported attendance equation to arrive at the "*instrumental variables"* estimate of the causal effect of the *Boîtes* exposure, "purged" of the potential bias that may have resulted from the original correlation between A_i and ε_i.[13]:

$$(8) \quad \Delta y_i = \Delta\beta_0 + \beta_1 A_i^* + \beta_k Z_k + \Delta\varepsilon_i$$

Importantly, the instrumental variables procedure does not provide an estimate of the causal effect of *Boîtes* exposure for *all* individuals who would potentially attend the events. Rather, the estimated effect is limited to those individuals who attended the session *as the result of the randomized encouragement* itself, i.e., those individuals who were exogenously "pushed" into *Boîtes* exposure. This group constitutes 20% of individuals in treatment villages (as noted above, calculated by subtracting the 61% of attendees in the non-encouragement group from the 82% of attendees in the encouragement group). We cannot estimate the effects of the session on individuals who would *always* have attended, whether or not they were encouraged, nor the effects on individuals who would *never* have attended, regardless of encouragement. The so-called "Local Average Treatment Effect" from the instrumental variables model, nevertheless, is informative substantively, as it provides a causal effect of the treatment on individuals who are pushed into attendance as a result of a relatively mild effort at recruitment, which mimics to some extent the kind of mobilization from NGOs or other civil society organizations that typically precedes a civic education event.[14]

3. Additional Explorations

The foregoing analysis form the core of our concerns in the study: estimating the overall village-level effects of the *Boîtes* events, and estimating the effects of exposure to the events among those individuals who attended the *Boîtes* sessions (or, in the case of the instrumental variables model, those individuals who attended as a result of the "push" from the encouragement

[13] One additional assumption of the IV procedure is that the randomized encouragement variable cannot have a *direct* effect on the outcome variables, as in that case the second stage estimation procedure will break down completely (given that A* as well as all of the variables that predict A* would all be included in the outcome equation). This assumption would be violated if the encouragement to attend the *Boîtes* session prompted individuals to learn about decentralization, independent of attending the event. This seems unlikely to have occurred, given the objective lack of progress in the actual implementation of the decentralization process in the DRC and the relatively low salience of the issue at the time of the study.

[14] We use CEM matching to balance the "encouraged" and "non-encouraged" individuals on relevant covariates before the instrumental variables estimation. We also include the village-level treatment/control variable in the first stage estimation in order to arrive at a more accurate prediction of self-reported attendance, though none of the substantive conclusions we draw depend on the inclusion of this variable.

manipulation). Subsequent analyses extend our focus to three other areas that are also important for the overall evaluation of the *Boîtes* events.

First, we seek to assess the extent to which the *Boîtes* events generated "spillover" or "secondary" effects among non-attendees in the treatment villages. We accomplish this by comparing the changes in decentralization orientations among the individuals in the control villages to different groups of individuals in the treatment villages. An initial comparison could be between the control villages and those in the treatment villages who were randomly *not encouraged* to attend; this comparison would yield a kind of "intent to treat effect" (or more precisely, the intent "not to treat" effect). Yet we know that some 60% of individuals in the not-encouraged group from treatment villages nevertheless took up the treatment, so we cannot legitimately claim that effects using this method would produce estimates of "spillover". We conduct instead a set of analyses comparing the self-reported non-attendees in treatment villages to individuals in control villages, and attempt as best as possible to take the self-selected nature of individuals' non-attendance into account. That is, individuals in the treatment villages who opted out of the treatment are likely to be different from the control individuals on factors that may affect changes in the outcome variables over time, regardless of whether they came into contact with or otherwise learned about the messages of the *Boîtes* sessions. We do not have an instrumental variable available to deal with this potential bias directly, but we are able to match the non-attendees to the control respondents on as many observed covariates as possible through the CEM procedures described above. This produces an estimate of spillover among non-attendees who are balanced as fully as possible with control respondents on demographic, political, and motivational factors that may have otherwise confounded the comparisons, as well as balanced on the pre-*Boîtes* levels of each dependent variable. However, the estimates of spillover within the treatment villages are not as precise as they might be, due to the relatively small number of individuals in our sample who did not attend the *Boîtes* events (N=252).

Our second set of additional analyses concerns possible differences in the causal effects of the *Boîtes* "treatments" depending on factors related to the *quality* and *timing* of the sessions. Specifically, we hypothesize that *Boîtes* events that were led by more competent facilitators, that contained more complete information, and that were better organized had stronger effects on individual decentralization orientations than events that were of lesser "quality". To test this hypothesis, we extend the initial village level model in equation (4) by including a variable that interacts the treatment village indicator variable with *Session Quality*, the variable discussed above that represents the average level of satisfaction with the event on the competence, information, and facilitator dimensions among individuals who attended the event.

We test further for the possibility of "fade-out" effects of the *Boîtes* events, such that individuals interviewed immediately after the event took place may show greater effects than individuals interviewed some weeks later. These models are estimated by interacting the treatment village indicator variable with the length of time since the *Boîtes* event in the respondent's village.

Finally, we test whether the *Boîtes* sessions had "heterogeneous effects," that is, differential effects on different kinds of individuals, in particular whether the sessions had greater effects on those with lower levels of political and social resources, or on those with lower initial levels of the decentralization knowledge and other outcomes of interest. All of these models were

estimated by constructed interaction variables with the treatment indicator and including these interactions in the equation (4) above.

Chapter III. The Impact of the *Boîtes à Images* Sessions: Basic Results

A. Subjective Assessments of Session Quality

We begin our assessment of the *Boîtes à Images* sessions by presenting the subjective views of the program among individuals who attended the events in the treatment villages. Judged according to this criterion, the events were highly successful. The figures below show that feelings of satisfaction with the sessions were nearly universal among the participants.

Figure III-1a

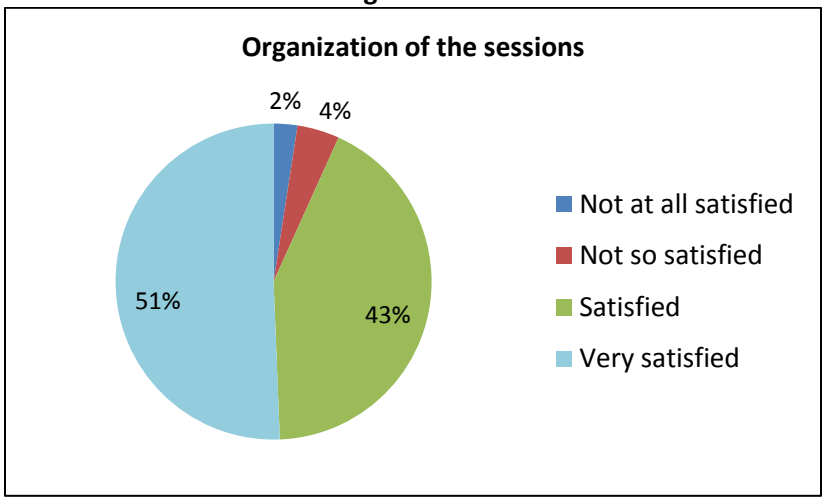
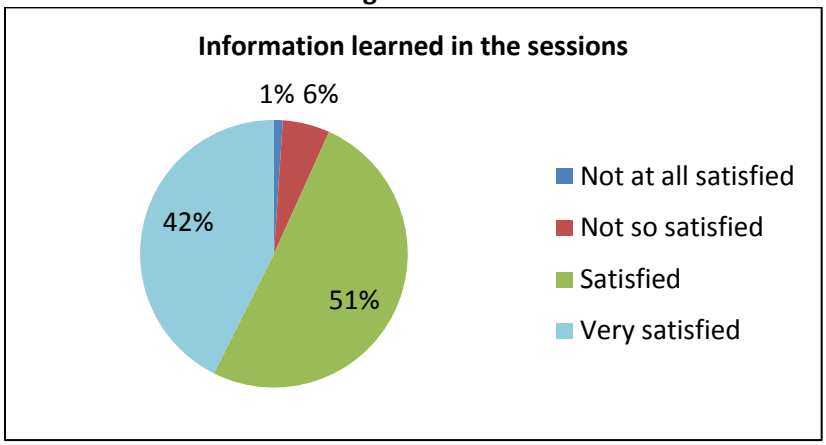


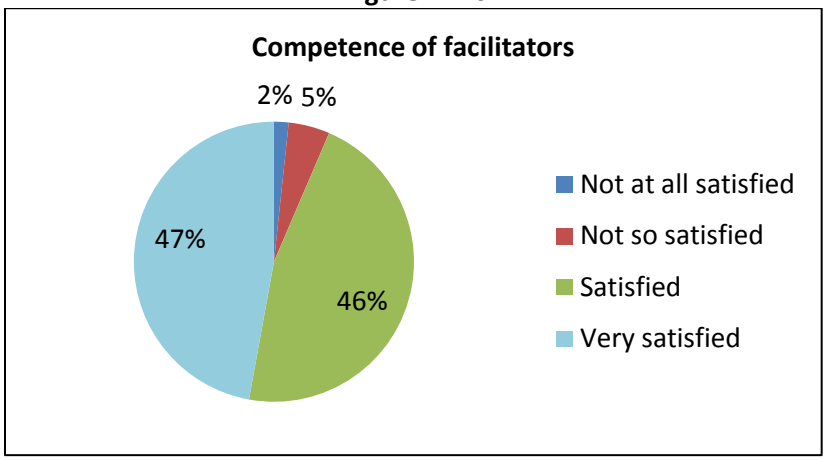
Figure III-1b



For instance, half of the participants report being "very satisfied" with the organization of the session (Figure III-1a), and some 43% more report being "satisfied" on this dimension. Similar distributions —approximately half being "very satisfied" and half being "satisfied" are seen for satisfaction with the "information learned during the session" and the "competence of the facilitators", with very few responses on any of the questions being registered in the "dissatisfied" categories. These results suggest that the *Boîtes à Images* events were popular

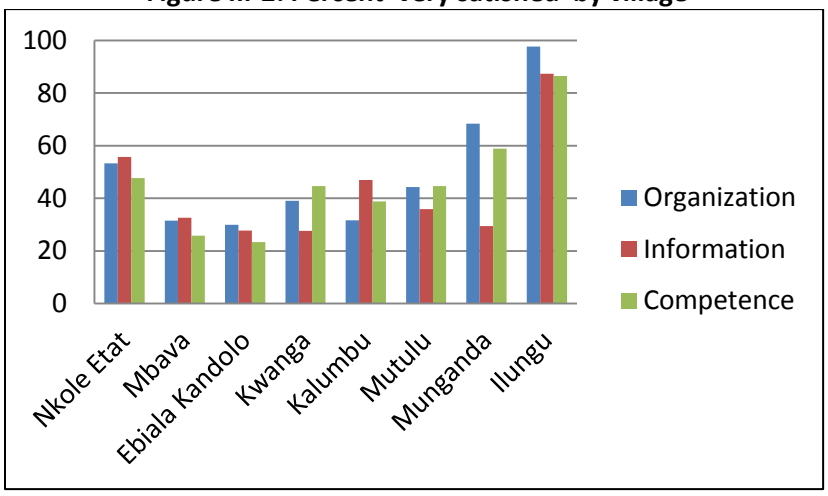
among those taking part, with a high degree of satisfaction with the sessions' organization and facilitators, and a high degree of perceived effectiveness in terms of transmitting information.

Figure III-1c



It should be noted, however, that the level of satisfaction differs considerably across participants from the eight different events. Though levels of "dissatisfaction" are never more than 10% in any of the locales, the levels of "very satisfied" versus "satisfied" respondents do show substantial variation. Figure III-2 shows the proportion of "very satisfied" respondents across the eight locations for each of the three satisfaction questions. As can be seen, proportions of respondents who were "very satisfied" with the organization of the session ranged from a high of 97% in Ilungu (in Inongo *Territorie*) to a low of 30% in Ebiala Kandolo (in Idiofa *Territorie*).

Figure III-2: Percent 'very satisfied' by village



Variations of similar magnitude are seen for proportions very satisfied with "information learned" and "facilitator competence": the event in Ilungu rated highest, the events in Ebiala Kandolo and Mbaya generally the lowest, with the other events falling somewhere in between. It is also the case that events rated higher on one dimension generally show higher values on the other dimensions as well: the average correlation between the measures is approximately .40, and the reliability coefficient (alpha) is approximately .66. We conclude, then, that

satisfaction with the events among participants was generally quite high, but that there was nevertheless significant variation across the eight events in terms of perceived quality, organization, and effectiveness of the sessions. We investigate in section E below the degree to which these "quality" dimensions may have been related to the strength of the sessions' impacts on individuals' decentralization orientations.

B. Village-Level Effects of the *Boîtes* Sessions

Demonstrating the effectiveness of the *Boîtes à Images* events, however, must go beyond whether attendees were satisfied with the session or with what they believe they learned. The core analyses of the study attempt to determine whether the events led to significantly greater changes in decentralization knowledge, attitudes, and political engagement among individuals in villages where the *Boîtes* events took place compared to individuals in comparable control villages and to determine the magnitude of the effects on the specific individuals who attended the events as well. We begin by showing the distribution and changes in all the orientations from the baseline to the post-test wave of observation for all individuals in the control and treatment villages. Table III-2a shows the results for the series of questions related to knowledge of the decentralization process, perceived knowledge of decentralization, general political knowledge, and perceived knowledge of the DRC constitution; Table III-2b shows the results for other decentralization orientations examined in the study, including support for the process, opinions about the pace of decentralization, support for greater responsibility of local governments in a series of policy areas, and discussion about the issue. Table III-2c shows the results for more general attitudes about democracy and overall political engagement. These comparisons represent the "raw" differences between the treatment and control villages *before* implementing the CEM matching and balancing procedures in subsequent regression analyses.

Table III-2a shows the percentages of individuals in the baseline and post-*Boîtes* surveys who answer each of the decentralization knowledge questions correctly. It can be seen that 11% more individuals in control villages showed increase over time in correct responses to the general question about decentralization giving less power to the central government, with a comparable 11% change in the increase in correct responses among individuals in treatment villages. On all other decentralization knowledge questions, however, there were dramatically greater changes in correct responses among individuals where *Boîtes à Images* events took place than in the comparable control villages. For example, in the post-*Boîtes* wave of observation, roughly 60% of individuals in the treatment villages correctly identified the *Caisse Nationale de Péréquation* and correctly noted the proportion of funds that would be coming to localities from the central government; these represented increases of approximately 40% from the baseline survey on these items, compared to changes of approximately 10% in control villages. Changes in treatment villages for correct responses on the other decentralization knowledge questions, related to the number of provinces envisioned in the decentralization process and on who would execute the laws in la *Chefferie*, were also of much greater magnitude in treatment than control villages over time.

We show the general differences between changes in decentralization knowledge for the treatment and control villages by constructing a summary scale of the number of correct responses given by the individual on the five items. As can be seen, the proportion who respond correctly on 3 or more of the 6 items changes by only about 10% among control village individuals (from 38% to 46%), while the corresponding change is 35% among individuals in

villages where *Boîtes à Images* events took place. The effect is even more dramatic if one examines the proportions of individuals who increased *at all* in their decentralization knowledge over time (i.e., ignoring individuals whose knowledge stayed the same or decreased from the baseline to the post-*Boîtes* wave of observation). In the control villages, just over 1/3 of all individuals (36%) registered some increase over time; this figure nearly doubles to 69%, or more than 2/3 of all individuals in treatment villages. These are large differences, and strong initial evidence that the *Boîtes* events were effective in their primary goal of educating individuals about the decentralization process.

Table III-2a. Decentralization and General Knowledge Pre- and Post-*Boîtes Sessions*

	Control Villages			**Treatment Villages**		
Variable	Pre	Post	Δ	Pre	Post	Δ
Decentralization gives less power to the central gov't in Kinshasa	42.7%	52.9%	10.2%	53.0%	63.5%	10.5%
Decentralization gives more power to the provinces and local gov't	41.8%	43.3%	1.5%	47.8%	59.8%	12.0%
No. of provinces after decentralization	52.9%	62.6%	9.8%	52.4%	81.9%	29.5%
Who makes the laws of the chiefdom	38.9%	42.9%	4.0%	40.1%	54.5%	14.4%
Proportion of funds for provinces and local gov't coming from central gov't	15.9%	25.3%	9.3%	23.5%	61.9%	38.3%
Caisse Nationale de Péréquation funds local development projects	3.4%	13.8%	10.4%	13.4%	58.6%	45.1%
No. of correct responses to decentralization questions	0 = 16.6% 1 = 23.4% 2 = 24.6% 3 = 21.0% 4+ = 14.4% ($\overline{X}$ = 1.96)	0 = 7.2% 1 = 21.0% 2 = 26.1% 3 = 22.9% 4+ = 22.7% ($\overline{X}$ = 2.41)	0.45	0 = 11.7% 1 = 20.2% 2 = 25.0% 3 = 21.4% 4+ = 21.7% ($\overline{X}$ = 2.30)	0 = 4.7% 1 = 6.9% 2 = 10.5% 3 = 19.2% 4+ = 58.7% ($\overline{X}$ = 3.80)	1.50
Perceived knowledge of decentralization	1 = 15.3% 2 = 18.3% 3 = 38.1% 4 = 28.4% ($\overline{X}$ = 2.80)	1 = 14.7% 2 = 11.1% 3 = 42.6% 4 = 31.7% ($\overline{X}$ = 2.91)	.12	1 = 15.8% 2 = 14.3% 3 = 35.0% 4 = 34.9% ($\overline{X}$ = 2.89)	1 = 7.0% 2 = 7.9% 3 = 33.3% 4 = 51.8% ($\overline{X}$ = 3.30)	.42
General political knowledge (No. of correct responses)	0 = 3.0% 1 = 20.6% 2 = 41.4% 3 = 28.7% 4 = 6.4% ($\overline{X}$ = 2.15)	0 = 4.5% 1 = 22.1% 2 = 34.4% 3 = 33.8% 4 = 5.3% ($\overline{X}$ = 2.13)	-.01	0 = 6.1% 1 = 23.2% 2 = 36.5% 3 = 28.4% 4 = 5.8% ($\overline{X}$ = 2.05)	0 = 3.6% 1 = 14.6% 2 = 35.3% 3 = 34.8% 4 = 11.6% ($\overline{X}$ = 2.36)	.31
Perceived knowledge of the constitution	0 = 57.3% 1 = 29.3% 2 = 13.4% ($\overline{X}$ = 0.56)	0 = 55.4% 1 = 30.4% 2 = 14.2% ($\overline{X}$ = 0.59)	.03	0 = 48.0% 1 = 32.7% 2 = 19.4% ($\overline{X}$ = 0.71)	0 = 34.9% 1 = 41.1% 2 = 24.1% ($\overline{X}$ = 0.89)	.18
N of Cases	464 to 471			1,039 to 1,101		

Table III-2a also shows that individuals in treatment villages showed somewhat greater changes in general political knowledge than did those in control villages, and somewhat greater increases in their perceived knowledge of decentralization and constitutional issues in the DRC. For example, on the general factual knowledge scale, the proportions of individuals in the control villagers who answered 3 or 4 of the 4 questions correctly increased by 4%; this figure was 12% in the treatment villages. The proportion of control villagers who believe they are "very" or "somewhat informed" about decentralization and about the DRC constitution does not change much over time, while perceived knowledge on both dimensions rise by 13-15% in treatment villages. These differences, however, are much lower in magnitude than those observed on the decentralization knowledge questions. Still, they provide some initial suggestion that the *Boîtes* events may have stimulated general gains about decentralization, political and constitutional knowledge.

Table III-2b. Attitudes Toward Decentralization Pre- and Post-*Boîtes Sessions*

	Control Villages			**Treatment Villages**		
Variable	Pre	Post	Δ	Pre	Post	Δ
General support for decentralization	78.8%	86.0%	7.2%	74.9%	87.3%	12.4%
Positive aspects of decentralization	0 = 8.7% 1 = 15.1% 2 = 24.4% 3 = 51.8% ($\overline{X}$ = 2.19)	0 = 2.3% 1 = 8.3% 2 = 20.8% 3 = 68.6% ($\overline{X}$ = 2.56)	.36	0 = 8.2% 1 = 12.0% 2 = 23.8% 3 = 56.0% ($\overline{X}$ = 2.28)	0 = 3.5% 1 = 5.7% 2 = 17.1% 3 = 73.7% ($\overline{X}$ = 2.61)	.33
Negative aspects of decentralization	0 = 50.3% 1 = 32.3% 2 = 17.4% ($\overline{X}$ = 0.67)	0 = 47.8% 1 = 24.0% 2 = 28.2% ($\overline{X}$ = 0.80)	.13	0 = 48.2% 1 = 31.3% 2 = 20.4% ($\overline{X}$ = 0.72)	0 = 61.4% 1 = 20.6% 2 = 18.0% ($\overline{X}$ = 0.57)	-.16
Pace of decentralization is too slow	62.4%	63.5%	1.1%	61.9%	58.5%	-3.4%
Decentralized policy responsibility: military and police	0 = 66.7% 1 = 22.1% 2 = 11.3% ($\overline{X}$ = 0.45)	0 = 64.8% 1 = 25.7% 2 = 9.6% ($\overline{X}$ = 0.45)	.00	0 = 73.0% 1 = 18.3% 2 = 8.7% ($\overline{X}$ = 0.36)	0 = 82.8% 1 = 11.2% 2 = 6.0% ($\overline{X}$ = 0.23)	-.13
Decentralized policy responsibility: roads and transportation	30.6%	41.5%	10.9%	27.4%	21.7%	-5.7%
Decentralized policy responsibility: schools and education	32.5%	41.0%	8.3%	40.1%	37.1%	-2.7%
Decentralized policy responsibility: taxation	46.3%	58.9%	12.3%	56.9%	53.0%	-4.2%
N of Cases	463 to 471			1,078 to 1,101		

However, similar gains were not observed on orientations aside from decentralization and political knowledge. As Table III-2b shows, the changes in *attitudes* about decentralization observed over time in control and treatment villages were all relatively small. Individuals in both control and treatment villages were highly supportive of decentralization at both waves of observation, with the treatment villages showing only a slightly larger increase. Both groups

increased modestly in their views of the positive aspects of decentralization, and both groups stayed roughly the same on their views of the negative aspects of decentralization and their (generally negative) views of the pace with which decentralization was going in the country. There were some modest changes in the groups' views on which level of government should be responsible for different policy areas, with individuals in the treatment villages being more likely over time to ascribe greater responsibility for the national government for issues such as police and the military, while individuals in the control villages were more likely over time to ascribe greater responsibility for local governments on issues such as schools and taxation. All of this indicates that, whether or not a *Boîtes* event took place in the village, changes on decentralization attitudes were, for the most part, generally minimal.

Table III-2c. Democratic Attitudes and Political Engagement Pre- and Post-*Boîtes Sessions*

	Control Villages			**Treatment Villages**		
Variable	Pre	Post	Δ	Pre	Post	Δ
Democracy is the best option	79.4%	78.8%	-0.1%	75.4%	80.1%	4.7%
Satisfaction with democracy	0 = 54.7% 1 = 34.6% 2 = 10.7% ($\overline{X}$ = 0.56)	0 = 49.5% 1 = 37.2% 2 = 13.4% ($\overline{X}$ = 0.64)	.07	0 = 36.1% 1 = 39.3% 2 = 24.6% ($\overline{X}$ = 0.88)	0 = 34.4% 1 = 47.6% 2 = 18.0% ($\overline{X}$ = 0.84)	-.04
Political tolerance	0 = 26.8% 1 = 29.3% 2 = 44.0% ($\overline{X}$ = 1.17)	0 = 20.4% 1 = 28.7% 2 = 51.0% ($\overline{X}$ = 1.31)	.13	0 = 22.6% 1 = 28.0% 2 = 49.4% ($\overline{X}$ = 1.27)	0 = 13.9% 1 = 17.3% 2 = 68.9% ($\overline{X}$ = 1.55)	.28
Political efficacy	0 = 15.1% 1 = 42.9% 2 = 42.0% ($\overline{X}$ = 1.27)	0 = 10.6% 1 = 43.5% 2 = 45.9% ($\overline{X}$ = 1.35)	.08	0 = 14.7% 1 = 46.9% 2 = 38.4% ($\overline{X}$ = 1.24)	0 = 13.2% 1 = 48.0% 2 = 38.9% ($\overline{X}$ = 1.26)	.02
Political discussion	0 = 49.0% 1 = 20.0% 2 = 31.0% ($\overline{X}$ = 0.82)	0 = 53.5% 1 = 17.6% 2 = 28.9% ($\overline{X}$ = 0.75)	-.07	0 = 49.0% 1 = 20.2% 2 = 30.8% ($\overline{X}$ = 0.82)	0 = 46.0% 1 = 26.6% 2 = 27.4% ($\overline{X}$ = 0.81)	.00
Political participation	0 = 22.9% 1 = 50.3% 2 = 17.8% 3 = 8.9% ($\overline{X}$ = 1.13)	0 = 26.5% 1 = 53.1% 2 = 15.5% 3 = 4.9% ($\overline{X}$ = 0.99)	-.14	0 = 21.4% 1 = 51.5% 2 = 20.5% 3 = 6.5% ($\overline{X}$ = 1.12)	0 = 23.8% 1 = 52.9% 2 = 15.2% 3 = 8.2% ($\overline{X}$ = 1.08)	-.04
N of Cases	468 to 471			1,056 to 1,101		

Table III-2c shows even smaller differences in the gains observed in control and treatment villages for political orientations not directly related to decentralization. Changes in control and treatment villages were small and of similar magnitude in treatment and control villages in terms of individuals' support for democracy (very high at both time periods in both sets of villages), their satisfaction with the way democracy is going in the DRC (mixed views in both sets of villages), their levels of political and social tolerance (mixed with slightly higher gains in the treatment villages), and in terms of cognitive and behavioral political engagement (virtually identical values in treatment and control villages over time). We conclude that, whatever effects the *Boîtes* sessions had on political knowledge, they appear not to be associated with general gains in democratic orientations or engagement with the political process.

We estimate the effects of the *Boîtes* sessions at the village level more formally through the regression models outlined in Chapter II (equation 4). In these models, we predict changes in each orientation over time with an indicator variable signifying whether the individuals was in a treatment or control villages, with the estimation sample of treatment and control village individuals being balanced through the CEM procedure on the initial value of each orientation (so as to equate the treatment and control groups' "starting points" in assessing change). We also balance the villages on covariates such as age, sex, political interest and media attentiveness so as to minimize these variables' potentially confounding biases. The coefficients can then be interpreted as the effect of residing in a treatment village on an individual's change in each orientation over time, compared to an individual with similar initial levels of the orientation and with similar demographic and political characteristics who resides in a control village. We present two models: one with only the "treatment" variable included, and one with the treatment variable along with additional control variables -- the initial level of the orientation and the other covariates that went into the CEM balancing process. There are virtually no differences between the estimates obtained from the two models, which gives us added confidence that the CEM matching procedure worked as expected in balancing the two samples on relevant covariates. The results are shown in Table III-3 above.

It can be seen that the patterns observed in the basic cross tabulations above are confirmed in the regression analyses. By far the strongest effect of the *Boîtes* sessions is observed for the decentralization knowledge scale, with individuals in treatment villages increasing by approximately 1 and 1/3 "correct" answers after the *Boîtes* event took place compared to comparable individuals in control villages. This is an effect of sizeable substantive magnitude as well, with the single exposure of the *Boîtes* event producing an effect that represents over 20% of the distance of the entire knowledge scale, and one that represents a change of nearly ¾ of a standard deviation on the change in knowledge over time. To the best of our knowledge, this is one of the largest effects registered in the several civic education evaluation studies previously conducted on behalf of USAID, and certainly the largest registered on political knowledge (Finkel 2003; Finkel and Smith 2011; Finkel *et al.*, 2012). The *Boîtes* sessions do appear to have stimulated substantial gains in knowledge about the decentralization process at the village level from the baseline wave of observation to the post-*Boîtes* interviews.[15]

The results in the rest of the table also largely replicate the patterns seen in the cross-tabulations. Significant effects of residing in a treatment village are observed for general knowledge and perceived knowledge of the constitution, though the effects are of substantially smaller magnitude than for decentralization knowledge (with the effects for both variables representing distances of approximately 10% on the knowledge scales and approximately .30 in standard deviation terms).[16]

But the effects on decentralization and overall political knowledge are not associated with significant effects on *any* of the other variables tested in the study, save for two small effects on attitudes towards decentralized authority in specific policy areas. Individuals in the treatment

[15] The effect remains strongly significant in the randomization inference robustness check ($p<.01$).
[16] The randomization inference test indicates further that only the general knowledge effect remains significant in this robustness check, though perceived knowledge of decentralization and perceived knowledge of the constitution are borderline in statistical significance ($p=.07$ and .08, respectively).

villages are more likely to favor national authority in the areas of police and military *and* more likely to favor national authority in the areas of roads and transportation as well.

Table III-3 Village-Level Effects of *Boîtes Sessions*

Variable	w/o controls	w/ LDV and controls
Overall decentralization knowledge	1.32*** (.33)	1.38*** (.27)
Perceived knowledge of decentralization	.32* (.12)	.35** (.12)
General political knowledge	.32** (.10)	.30* (.12)
Perceived knowledge of the constitution	.27* (.11)	.27* (.10)
General support for decentralization	.07 (.11)	.06 (.12)
Positive aspects of decentralization	.08 (.16)	.08 (.08)
Negative aspects of decentralization	-.17 (.19)	-.17 (.18)
Pace of decentralization is too slow	-.01 (.09)	.00 (.09)
Support for decentralization: military and police	-.18* (.08)	-.18 (.09)
Support for decentralization: roads and transportation	-.18** (.06)	-.18* (.05)
Support for decentralization: schools and education	-.09 (.07)	-.10 (.09)
Support for decentralization: taxation	-.10 (.08)	-.10 (.06)
Democracy is the best option	.01 (.06)	.01 (.04)
Satisfaction with democracy	.02 (.11)	.02 (.10)
Political tolerance	.22 (.17)	.23 (.17)
Political efficacy	-.08 (.12)	-.07 (.11)
Political discussion	-.04 (.11)	-.04 (.12)
Interest in politics	.17 (.14)	.17 (.14)
Political participation	.12 (.09)	.09 (.09)

All estimates are CEM-weighted with clustered robust standard errors in parentheses. N ranges from 1,305 to 1,515
***p < .001 (two-tailed); **p < .01 (two-tailed); *p < .05 (two-tailed)

These effects indicate that, to the extent that treatment villages showed any changes in their decentralization preferences after the *Boîtes* sessions, they tended in the direction of more *national* as opposed to local authority.[17] No other variable shows statistically significant differences between the changes observed in treatment and control villages over time. We conclude that there are strong and significant effects of the *Boîtes* sessions at the village level on knowledge about decentralization, weak but significant effects of the *Boîtes* sessions at the village level on general political knowledge and preferences for more national authority over roads and transportation, and essentially no effects on any other orientation regarding decentralization, democracy, or political engagement.

C. The Effects of Attending *Boîtes* Sessions

The village-level effects estimated above represent the *total* impact of the *Boîtes à Images* sessions on individuals in treatment villages, compared to demographically and political comparable individuals in control villages with similar baseline levels of each orientation. We can view these "total effects" as resulting from the session's immediate direct effect on individuals who attended the event, plus any diffusion of the information from the session among individuals in the village after the event took place. We seek in the next several sections to disentangle these effects, that is, to estimate the separate effects of *direct* Boîtes exposure from effects on non-attendees from post-*Boîtes* discussion and diffusion through village social networks. As described in Chapter II above, we estimate three different models, each with advantages and limitations, in order to assess the direct effect of *Boîtes* exposure: 1) the *intent-to-treat* model that uses the randomized "encouragement to attend" manipulation as the key independent variable; 2) the *self-reported attendance* model that compares individuals who attended with individuals who did not attend; and 3) the *instrumental variable* model that uses the randomized encouragement as an exogenous proxy for attendance so that the effects of attendance, purged of potential self-selection bias are estimated (for the subset of individuals who were "pushed" into the treatment by the encouragement manipulation). We show the results of these models in Table III-4.

The results of all of these analyses show consistent effects of attending the *Boîtes à Images* sessions on the individual's knowledge about the decentralization process. The *intent-to-treat* (ITT) estimate is 1.23, indicating that randomly "encouraged to attend" individuals improved on the decentralization knowledge scale by nearly 1 and ¼ point compared to randomly "not encouraged" individuals. This value is somewhat smaller than the overall village estimate in Table III-3, likely due to the large number of "non-compliers" in the comparison category, i.e., individuals who were not encouraged to attend in the treatment villages but nevertheless attended the event. When comparing "randomly encouraged" individuals *only* to individuals in the control villages, the intent to treat estimate rises to 1.62. This is strong initial evidence of direct *Boîtes* impact in stimulating individual learning about the decentralization process.

This conclusion is reinforced by the results obtained from the two other models. Individuals who report having attended the event (column 3 in Table III-4) register a full 1.61 greater change in decentralization knowledge over time compared to those who did not attend, with this difference again being statistically significant. As noted above, this estimate may be

[17] The randomization inference tests show that the police-military effect is borderline significant at the .08 level, while preferences for more national authority in transportation remains significant at the .05 level.

inflated, to the extent that individuals who self-selected into the event were already increasing on knowledge, or whose unobserved characteristics led to learning changes due to some other aspect of the environment aside from their having attended the *Boîtes* event.

Table III-4: Effects of *Boîtes* Session Attendance

Variable	Encouraged vs. All non-encouraged	Attendees vs. All non-attendees	IV w/ Two instruments
Overall decentralization knowledge	1.23*** (.28)	1.61*** (.23)	1.95*** (.32)
Perceived knowledge of decentralization	.26** (.08)	.33*** (.08)	.53*** (.13)
General political knowledge	.21 (.11)	.28** (.10)	.36* (.15)
Perceived knowledge of the constitution	.13 (.18)	.30** (.08)	.35*** (.10)
General support for decentralization	-.01 (.10)	.11 (.10)	.10 (.17)
Positive aspects of decentralization	.06 (.07)	.11 (.06)	.05 (.09)
Negative aspects of decentralization	-.11 (.13)	-.19 (.13)	-.30 (.19)
Pace of decentralization is too slow	-.03 (.06)	.06 (.07)	-.02 (.11)
Support for decentralization: military and police	-.15** (.05)	-.14* (.07)	-.25** (.09)
Support for decentralization: roads and transportation	-.10 (.06)	-.08 (.06)	-.24*** (.07)
Support for decentralization: schools and education	-.05 (.06)	-.03 (.09)	-.11 (.10)
Support for decentralization: taxation	-.06 (.05)	-.09 (.06)	-.12 (.07)
Democracy is the best option	-.05 (.04)	.02 (.04)	.00 (.05)
Satisfaction with democracy	-.03 (.06)	.04 (.09)	.00 (.09)
Political tolerance	.11 (.12)	.11 (.12)	.24 (.21)
Political efficacy	.03 (.06)	.03 (.09)	-.07 (.11)
Political discussion	.08 (.08)	.05 (.09)	.02 (.13)
Interest in politics	.02 (.08)	.25* (.12)	.16 (.15)
Political participation	.14* (.06)	-.02 (.09)	.17 (.10)

All estimates are CEM-weighted with clustered robust standard errors in parentheses. N ranges from 1,329 to 1,512.
***p < .001 (two-tailed); **p < .01 (two-tailed); *p < .05 (two-tailed)

The *instrumental variables* estimate in column 4 of the table, however, shows that among the "purely exogenous" portion of *Boîtes* attendance, that is, the portion of the treatment village individuals who were affected by the randomization manipulation, the *Boîtes* effect is even larger, with a gain in knowledge of close to 2 more correct questions than among non-attendees. The results show, consistently and through multiple estimation procedures, that there were substantial direct effects of attending the *Boîtes à Images* sessions on individual learning about decentralization.

The table shows further that the same variables —and only those variables— that were significant at the village level in Table III-3 show consistently significant effects for *Boîtes* attendance as well. Individuals in treatment villages showed greater gains in general political knowledge, in perceived knowledge about decentralization and the DRC constitution, and greater support for national authority in the areas of police, military, and roads and transportation than individuals in control villages. These effects are largely replicated among *Boîtes* attendees. The self-reported attendance model (column 3) shows significant effects of attendance on general and perceived knowledge, support for national authority in policy and military matters, and overall political interest. Controlling for the self-selection effects in the *instrumental variables* model shows effects only on the knowledge and national authority support items, as the effects of attendance on political interest (and the *intent-to-treat* effect on political participation) are no longer significant. We note that the magnitude of all of these effects are relatively modest and substantially weaker than those observed for decentralization knowledge. There are no detectable effects of *Boîtes* attendance on any of the other orientations measured in the study.

D. "Spillover" Effects of the *Boîtes* Sessions on Treatment Village Non-Attendees

The results presented thus far allow us to draw initial conclusions about the extent to which the *Boîtes* sessions may have stimulated "secondary effects" in treatment villages, i.e., among individuals who did not attend the event but who may have learned from individuals in their families, neighborhood, work or friendships networks who did. We know from Table III-3, for example, that the overall village-level effect of the treatment on decentralization knowledge was 1.38, so that the average individual in the treatment villages increased on the decentralization knowledge scale by that amount compared to the average (comparable) individual in the control villages. We also know from Table III-4 that our best estimate of the direct effect of the session on those who attended model is somewhere between 1.61 and 1.95. Given our estimate of approximately 77% of the treatment village sample attended the event, we can then estimate the effect among the remaining 23% as being approximately zero, ranging from a high of .38 if the attendee effect is 1.61 to a low of -.52 if the attendee effect is 1.95. These calculations yield similar effects on the other knowledge-related variables that were shown to have significant village-level effects in Table II-3: general political knowledge (spillover effect ranging from .10 to .27); perceived knowledge of decentralization (spillover effect ranging from (-.19 to .42) to the constitution (spillover effect ranging from -.03 to .17). In nearly every case, the effect of the treatment on non-attendees is substantially smaller than the estimated direct effect on attendees, and in most cases we cannot rule out the possibility that there are no spillover effects whatsoever.

At the same time, we argued earlier that individuals who choose *not* to attend the sessions are different on many other relevant factors from self-selected attendees, and they may be very

different from an "average" individual in the control villages as well. We need as a further test to compare the changes in knowledge observed among this (self-selected) non-attendee group with as comparable a group of individuals in the control villages as possible. We therefore re-estimate the "difference-in-differences" between the sample of non-attendees and control villagers that are matched on all of the political and demographic covariates used in the study thus far (age, education, political interest, media attentiveness) and matched on the baseline level of each dependent variable.[18] We show these results in Table III-5 below.

Table III-5: Spillover Effects of *Boîtes Sessions*

Variable	Non-attendees vs. Control Village
Overall decentralization knowledge	.37 (.26)
Perceived knowledge of decentralization	.02 (.17)
General political knowledge	.15 (.13)
Perceived knowledge of the constitution	.06 (.11)
Support for decentralization: military and police	-.15 (.13)
Support for decentralization: roads and transportation	-.26*** (.05)

All estimates are CEM-weighted with clustered robust standard errors in parentheses. N ranges from 497 to 687.
***p < .001 (two-tailed); **p < .01 (two-tailed); *p < .05 (two-tailed)

The results show that there are no significant spillover effects on any of the variables tested, aside from support for national authority over roads and transportation. Our best estimate of the effect of the *Boîtes* session on non-attendees in treatment villages is .37 for the key dependent variable of decentralization knowledge, a reduction of some 80% from the effect observed on attendees. Reductions of upwards of 67-75% are seen on the other knowledge variables as well. We note that *if* these effects were statistically significant, it would provide some evidence of positive spillover, which would be notable, given that it would have resulted from single civic education event taking place in the village.

It may be that a larger study with more villages and more individuals who did not attend the event would provide an estimate with greater power. It may also be the case that spillover effects were present for a subset of non-attendees, e.g., family members or close friends of the participants. Again, a larger study with more detailed information on the social networks of the villagers, both attendees and non-attendees would be needed to make these determinations. For now, however, we conclude from the analyses that were possible to conduct with our data that spillover effects from the *Boîtes* sessions onto individuals who did not attend the sessions were negligible.

[18] As in previous analyses, the differencing procedure eliminates stable unobserved factors at both the individual and village level that may confound the inferences as well.

E. Effects of *Boîtes* Session Quality and Timing

We turn now to investigating the potential differential impacts of the *Boîtes* sessions, in this section focusing on the characteristics or timing of the events themselves. In Section F we explore potential differential effects of the sessions on different kinds of individuals. We focus on village-level impacts, as they are the most straightforward to estimate (following the models in Section A above), and they provide a sense of the total impact that different kinds of *Boîtes* sessions may have on individuals in treatment villages.

As noted above, we have two main hypotheses: that *Boîtes* sessions of higher "quality" – i.e., that are better organized, that have more competent facilitators, and that led to greater satisfaction on the part of the participants – would have stronger impacts than sessions that were of lower quality. We hypothesize further that some of the gains registered in treatment villages may potentially lessen over time, such that individuals in treatment villages whose post-*Boîtes* interviews took place closer to the event would show greater impacts than individuals in treatment villages who were interviewed well after the event took place. The results of both of these tests are of interest not only for assessing the *Boîtes* (VOICE) program, but also have important implications for the implementation of future civic education programs in the DRC and elsewhere.

1. Session Quality

We showed in Section A above that the events were rated as being of relatively high quality by participants: the sessions were thought to be well-organized, the facilitators were through to be competent, and there was general satisfaction with the information imparted during the sessions. But we also noted significant variation among the sessions in different villages as well. We therefore use the average value of the participants' ratings for each of the sessions as a proxy for overall *Session Quality*, and we exploit the variation in this measure across villages by interacting the quality variable with the treatment indicator variable to produce the following estimation equation:

$$(9)\ \Delta y_{ij} = \Delta\beta_0 + \beta_1 TREATMENT_i + \beta_2 TREATMENT_i * QUALITY_j + \beta_3 Z_i + \Delta\varepsilon_{ij}$$

where Z are control variables including the baseline value of the knowledge variable or other orientation tested.[19] It can be seen that the effect of the treatment will differ for every level of session quality to the extent that β_2 is statistically significant. It will also be possible with this model to provide a precise estimate of the effect of the treatment at each level of overall quality, and to determine whether this effect is statistically different from zero. We estimated this model for *all* orientations examined in the study, as it may be possible that significant effects would be registered at very relatively levels of session quality even for orientations that did not up until now show significant overall effects.

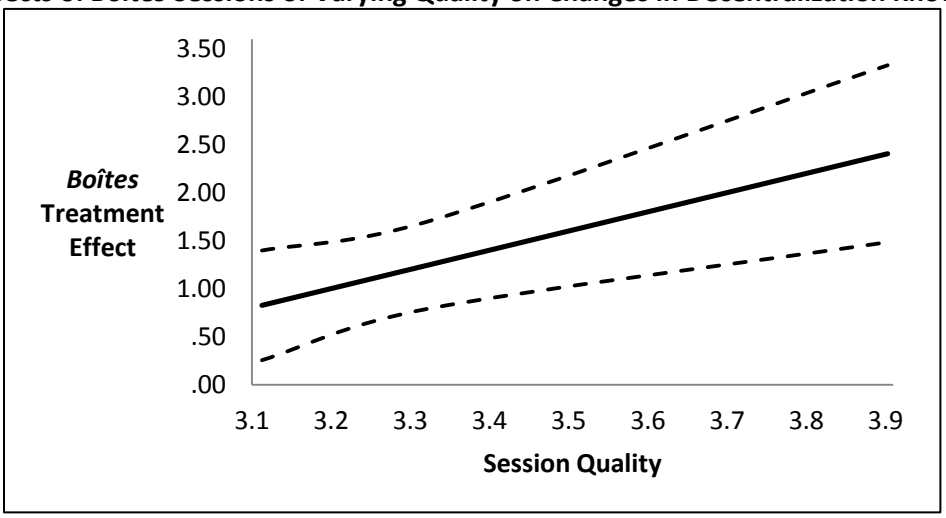
The estimation of this model results in several important findings. First, there is a strong, positive interaction effect for the key decentralization knowledge variable, indicating that individuals in treatment villages with higher quality *Boîtes* sessions learned significantly more

[19] As in previous analyses, the estimation sample is also "pre-balanced" on the covariates and baseline value of the dependent variables through the CEM procedure.

from the event, relatively to comparable individuals in control villages, than did individuals in treatment villages with lower quality sessions. We show this effect in Figure III-3 below, with the dark line representing the estimated treatment effect and the dotted lines representing the 95% confidence interval for these estimates.

It can be seen that the effect of the treatment is statistically significant at all levels of session quality, but that the magnitude of the effect rises considerably in villages with higher quality treatments. At the highest level of session quality, treatment villagers registered gains of nearly 2 ½ more correct responses on the decentralization scale than comparable control villagers, compared to gains of just less than 1 more correct response among treatment villagers where the session quality was lowest. This is strong evidence that proper implementation of the session through quality organization and competent facilitators matters greatly in determining the magnitude of the session's impact at the village level.

Figure III-3
The Effects of *Boîtes* Sessions of Varying Quality on Changes in Decentralization Knowledge



Second, there are generally *no* significant differences in the treatment effects on other decentralization orientations depending on the quality of the *Boîtes* session. There is some hint that the very highest quality treatments stimulate overall support for decentralization, but we cannot statistically rule out the notion that the differences in effects on this variable from sessions of different quality is zero. There is also some hint that sessions of higher quality lead to greater support for the current pace of decentralization, but regardless of session quality, all treatment effects for this variable are statistically indistinguishable from zero. The sole instance aside from overall decentralization knowledge where treatment effects do differ meaningfully depending on session quality is on the individual's perceived knowledge of the DRC constitution, and we show these results in Figure III-4 below. It can be seen that when session quality is lowest, the treatment effect is only .13, with this value being statistically insignificant. As session quality rises to its average level, the effect of the treatment doubles to a significant value of .24, and as session quality rises to its highest value, the effect of the treatment more than doubles from that amount to a significant .51 value. This represents a relatively large effect in standard deviation terms of .57.

The third, and perhaps most striking, result from these analyses is that there are consistently *positive* interactions of session quality on the treatment effects for the cluster of general political engagement variables. That is, sessions that are of higher quality stimulate greater, and in many cases, statistically significant gains in treatment villagers' political interest, participation, efficacy, and discussion, relative to comparable control villagers, than sessions of lower quality. We show these results in Figures III-5 to Figures III-8.

Figure III-4
The Effects of *Boîtes* Sessions of Varying Quality on Perceived Knowledge of the Constitution

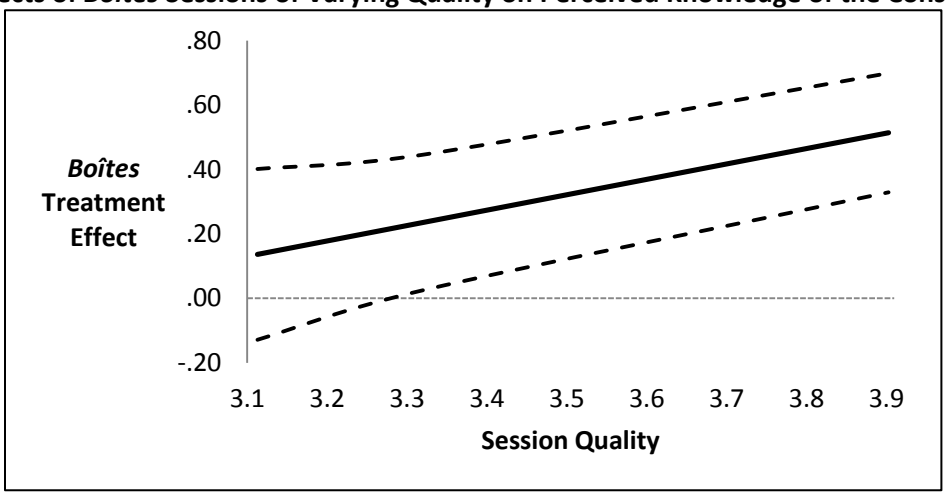
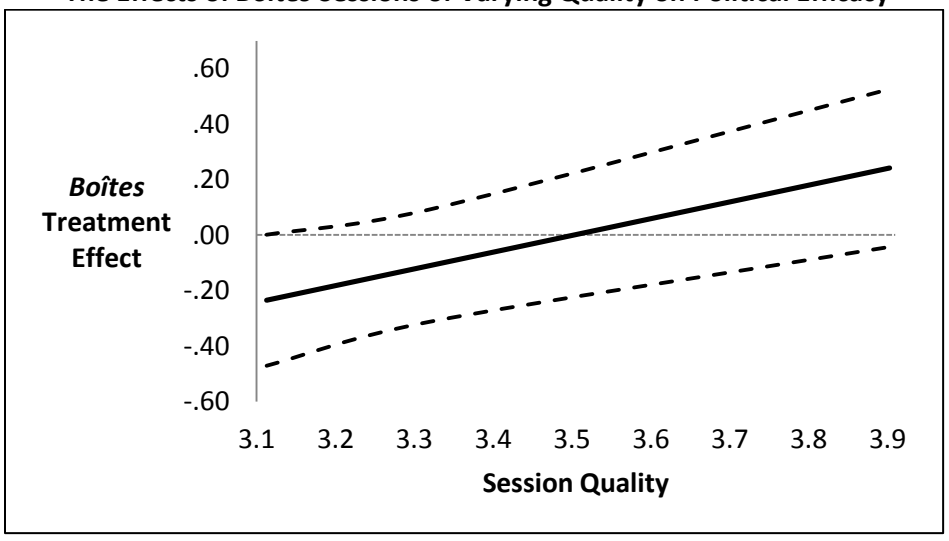


Figure III-5
The Effects of *Boîtes* Sessions of Varying Quality on Political Efficacy



The figures show consistently that high quality sessions affect the cluster of political engagement variables – the individuals' interest in politics, perceived competence in political, and levels of discussion and participation-- while low quality sessions do not. Sessions of the highest quality are associated with treatment effects of substantially greater magnitude than low quality sessions: .36 versus -.05 for participation, for example, .54 versus -.02 for interest, .24 versus -.24 for efficacy, and .20 versus -.17 for discussion. This indicates that, beyond the

effects that sessions may have on the immediate content of the curriculum, i.e. knowledge about the decentralization process, high quality delivery of the session also stimulates the individuals' general engagement with the democratic process. But these effects *only* took place in villages at relatively high levels of session quality, with few of the actual sessions examined in the study reaching those levels of quality necessary for these generalized effects to occur.

Figure III-6
The Effects of *Boîtes* Sessions of Varying Quality on Political Discussion

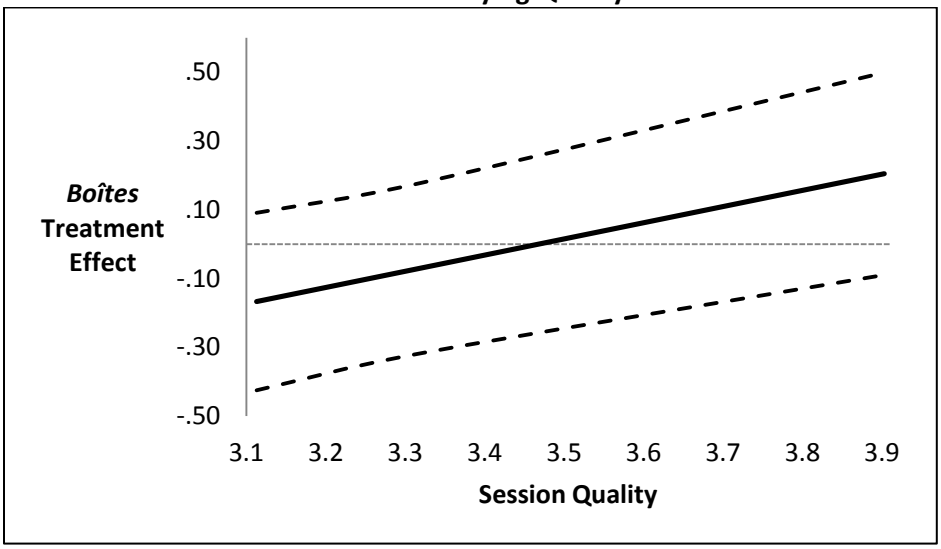


Figure III-7
The Effects of *Boîtes* Sessions of Varying Quality on Political Interest

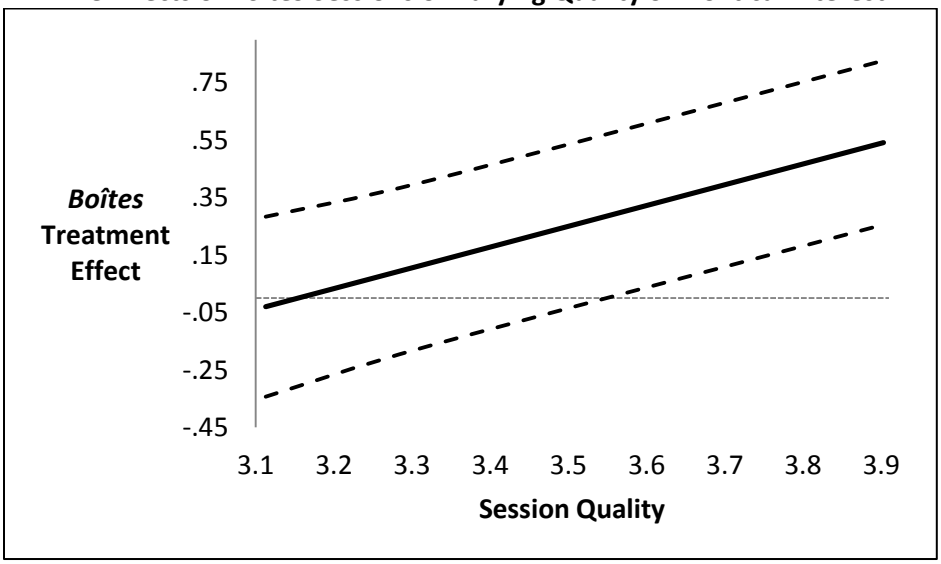
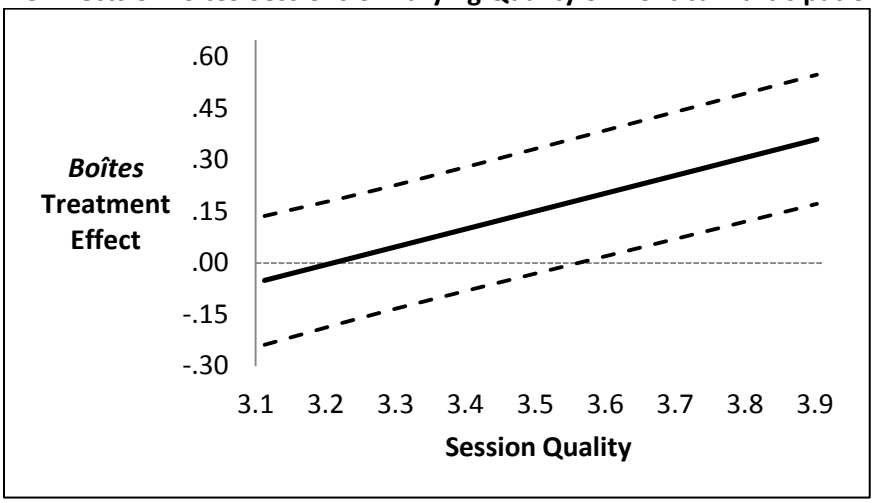


Figure III-8
The Effects of *Boîtes* Sessions of Varying Quality on Political Participation



2. Session Timing

We test the possibilities of "fade-out" effects here, such that the effects of *Boîtes* sessions may dissipate in the days and weeks following the event. The timing of the post-*Boîtes* interviews, as noted above, ranged from a few days to a just under a month, giving us some variation to exploit though not enough to determine truly longer-term effects of the sessions (see Finkel *et al.,* 2012). We estimate these models by interacting the treatment indicator variable with the days since the event took place, and including both of those variables in a model similar to equation (9) above for *Session Quality*. In these models, we also include a variable that measures the days between the baseline and post-*Boîtes* interviews, so that any tendency of the change in any of the orientations to occur for individuals in control village depending on the timing of the interview would also be taken into account (along with potential changes from the baseline interview to the date of the session for individuals in treatment villages as well).[20]

We estimated these models for all variables considered in the study. The results strongly parallel the findings from the *Session Quality* models: there are significant "fade-out" effects seen for the key knowledge variables related to the decentralization process and perceived knowledge of decentralization and the DRC constitution, and significant "fade-out" effects for the political engagement variables of interest and participation as well. We show these results in graph form in Figures III-9 to III-11 for the knowledge items, and Figures III-12 and III-13 for the engagement variables.

It can be seen in Figure III-9 that the positive effects of the *Boîtes* sessions on decentralization knowledge are much stronger immediately following the event than they are after one month has passed between the event and the wave 2 interview. Gains in correct knowledge after 3 days (the minimum value for this variable in the study) for individuals in treatment villages are nearly 2 items greater than for comparable individuals in control villages, while this effect falls

[20] As in previous analyses, other control variables were also included, and the treatment and control villages were balanced initially using the CEM procedures.

to 1.5 items greater for treatment villagers interviewed 10 days after the event, and drops by an additional 50% to .76 items greater for treatment villagers interviewed 26 days after the event (the maximum value for this variable in the study). This is evidence that the learning taking place as a result of the session, and post-session diffusion in the treatment villages, is much stronger in the days immediately following the session than it is nearly one month later. While the effect is statistically significant regardless of when the post-*Boîtes* interview took place, its magnitude drops by some 60% from the earliest post-*Boîtes* interviews to the latest. While we cannot extrapolate beyond the time period covered in the study, we can say that it does not appear that the *Boîtes* sessions produced as great a longer-term learning effect compared with its immediate one.

Figure III-9
The Effect of Time Elapsed Since *Boîtes* Session on Decentralization Knowledge

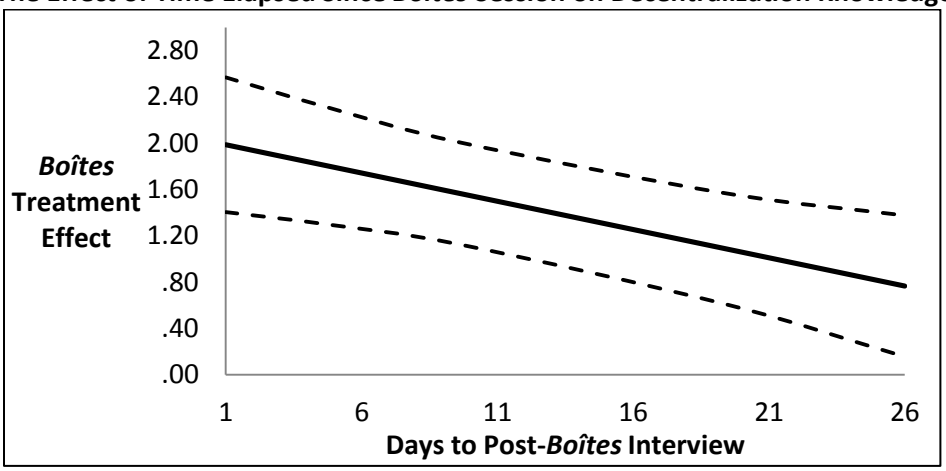


Figure III-10
The Effect of Time Elapsed Since *Boîtes* Session on Perceived Knowledge of Decentralization

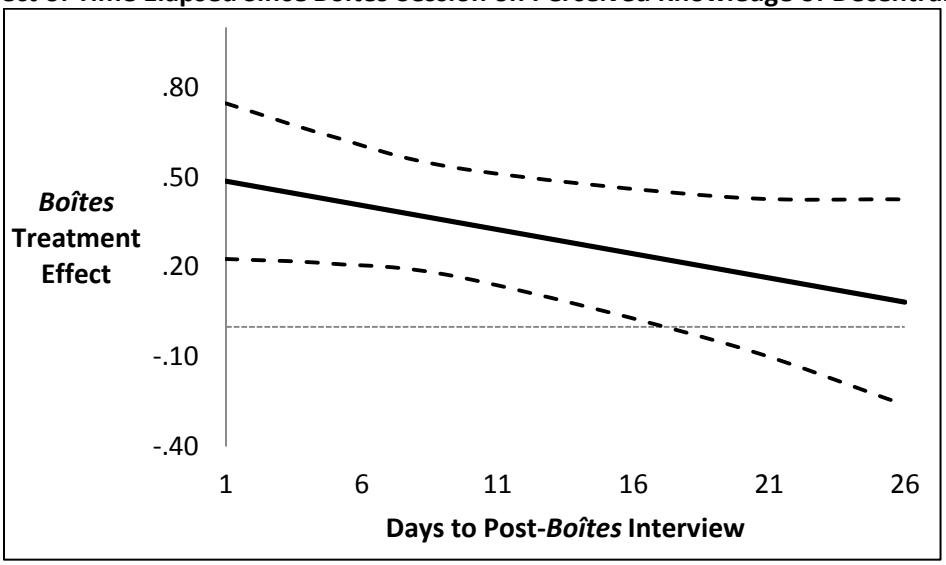


Figure III-11
The Effect of Time Elapsed Since *Boîtes* Session on Perceived Knowledge of the Constitution

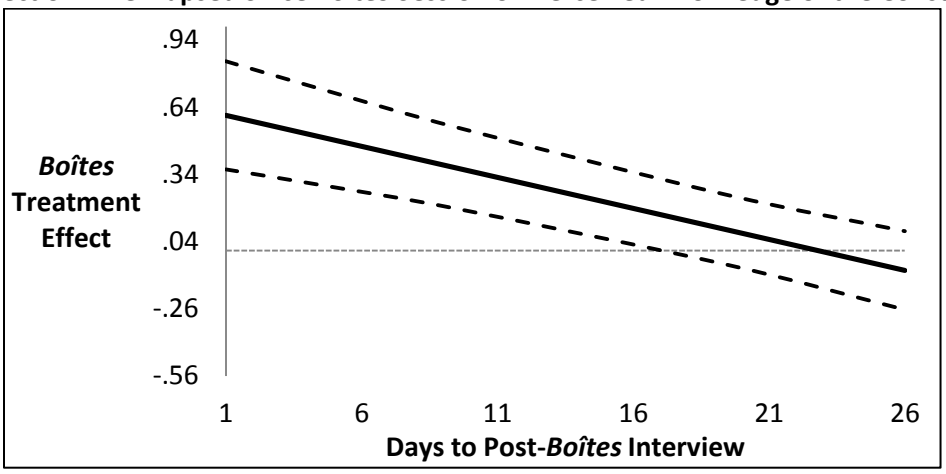


Figure III-12
The Effect of Time Elapsed Since *Boîtes* Session on Political Interest

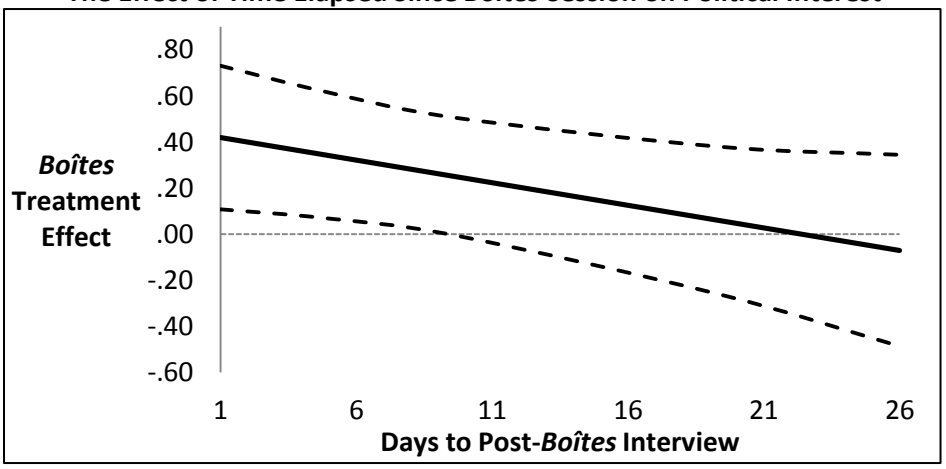
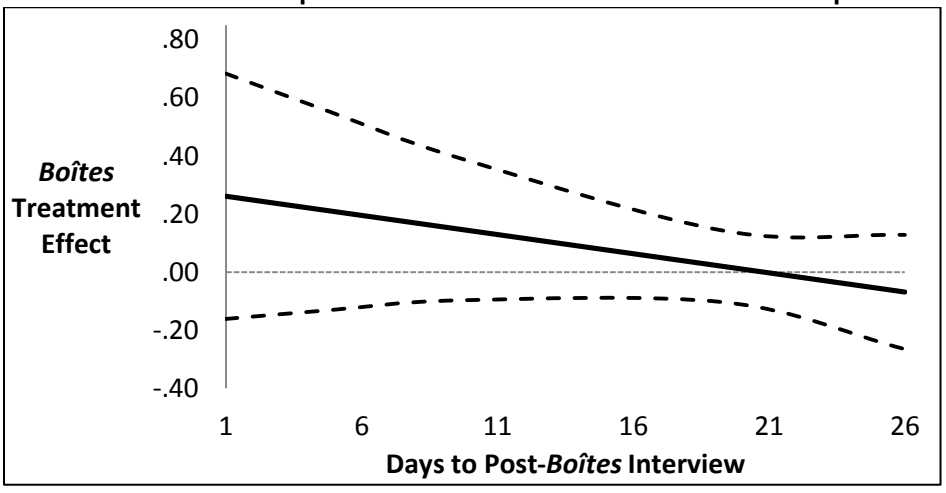


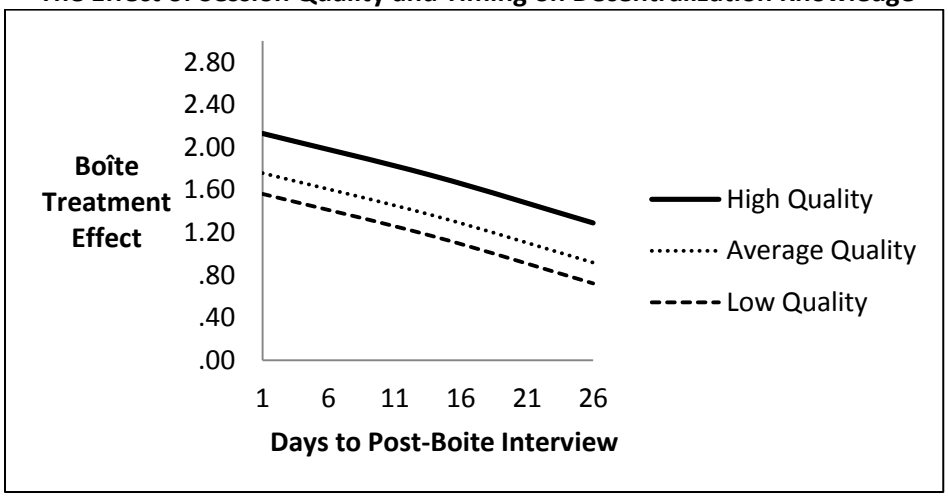
Figure III-13
The Effect of Time Elapsed Since *Boîtes* Session on Political Participation



An even steeper "fade-out" effect is shown for perceptions of knowledge about decentralization, and about the DRC constitution in Figures III-10 and III-11. Here the effects immediately following the sessions are estimated at approximately .60, more than double their "average" values as reported in Table III-3. But this effect falls to .35 by day 10, and becomes statistically insignificant and approaching zero by the third week after the session. This pattern suggests that the sessions provided an immediate boost in treatment individuals' perceptions of their competence about decentralization and constitutional issues, but that this boost was not manifest among individuals interviewed beyond the first few weeks post-treatment. These same patterns are shown in Figures III-12 and III-13 for political interest and participation: stronger effects in the immediate time period after the *Boîtes* session – effects that are much greater than the average effect seen in Table III-3– but weak and statistically insignificant effects by the third week post-treatment.

It is important to note that we cannot say with certainty that individuals are "forgetting" what they initially learned in the sessions, nor that there are initial boosts in efficacy and engagement that fade out over time. Since we do not have longitudinal data on the same individuals interviewed at different points post-treatment, we do not know what the effects may have been *for those individuals* as time passed after the *Boîtes* session took place. While the patterns are consistent with a "forgetting" process, they may also be consistent with other processes, for example, a correlation between session quality and post-session timing producing the "illusion" of a fade-out effect. In fact, the correlation between our "quality" and "time of post-*Boîtes* interview" among treatment individuals is -.72, indicating that sessions in which wave 2 interviews were conducted closer to the event were also rated more highly than when they were conducted several weeks later. This makes it very difficult to disentangle the independent effects of "quality" and "timing" in determining the magnitude of the *Boîtes* treatment's impact.

Figure III-14
The Effect of Session Quality and Timing on Decentralization Knowledge



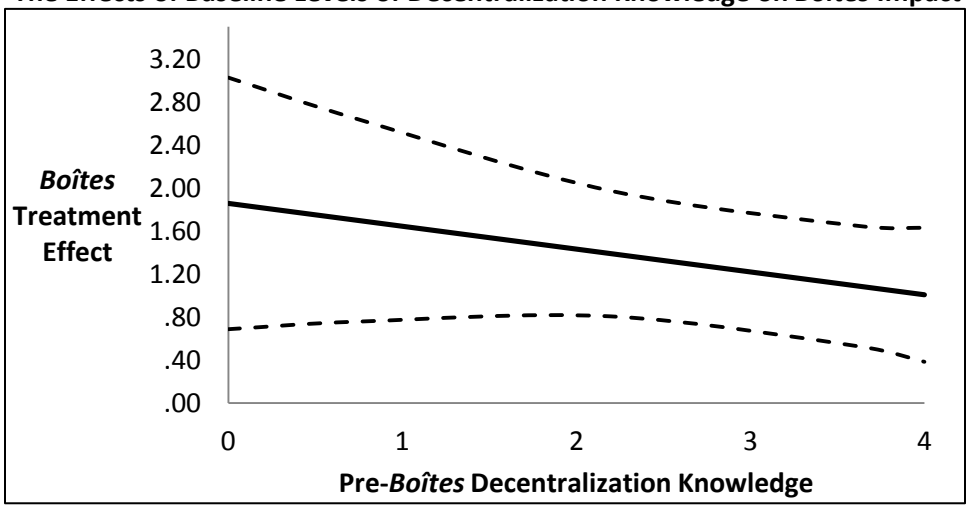
To the extent that we can estimate independent effects, however, it appears that both factors are important. In Figure III-14, we show the estimated effect of the *Boîtes* events on decentralization knowledge for sessions with different levels of quality and different times of the post-session interviews. It can be seen that, at every level of quality, individuals interviewed at a greater distance from the event show smaller gains, compared to comparable individuals in

control villages, than individuals interviewed closer to the event. It can also be seen that, at every time period following the event, sessions of greater quality produce greater gains in knowledge. Taken together, the two factors powerfully condition the effects of the treatment: when individuals are interviewed very close in time to when high quality sessions took place in their village, they show gains of over 2 correct scores on decentralization knowledge compared to comparable control villagers; but when individuals are interviewed weeks after a session of relatively lower quality, the effects fall by over 2/3 in terms of magnitude.

F. Effects of *Boîtes* Sessions on Different Kinds of Individuals

The final set of analyses tests for differences in the effects of the *Boîtes* events on different kinds of individuals. Previous evaluations suggest somewhat great support for the notion that exposure to civic education has greater effects on those individuals in greater "need", i.e., those less well-educated, less socially-connected, and, in the case of Kenya in 2007, those whose households had experienced post-election violence (Finkel and Smith 2011; Finkel *et al.*, 2012). These effects were generally not substantial, however, and other work has found support for civic education having differentially greater effects on political participation among those with *higher* levels of pre-existing political resources as well (Finkel 2003). We examine these processes here by interacting the "treatment" indicator with sex, education, and membership in secondary organizations to test whether *Boîtes* effects are larger for men in treatment villages than for women, those with more or less education, and those who belong to many versus few voluntary associations. We found *no* consistent evidence that the effects of the *Boîtes* sessions differed across these characteristics of individuals in treatment villages. With few noteworthy exceptions, there were similar effects (i.e., non-significant interactions) of the *Boîtes* session for treatment village men and women, for individuals with different educational attainment, and for individuals with varying levels of group memberships. We also examined effects for individuals who had "lost family members in the recent armed conflict" and found no differential impact compared to other treatment village individuals as well.

Figure III-15
The Effects of Baseline Levels of Decentralization Knowledge on *Boîtes* Impact



However, we did find suggestive evidence that the *Boîtes* treatment had greater effects on those with lower initial levels of decentralization and perceived knowledge of decentralization and the DRC constitution. We constructed interaction terms between the "treatment" indicator and the individual's baseline level of decentralization and perceived constitution knowledge, and showed in both cases that the effects of the *Boîtes* event were greatest among those with the lowest initial scores on each of the orientations. These effects are shown in graph form in Figures III-15, III-16, and Figure III-17.

Figure III-16:
The Effects of Baseline Levels of Perceived Decentralization Knowledge on *Boîtes* Impact

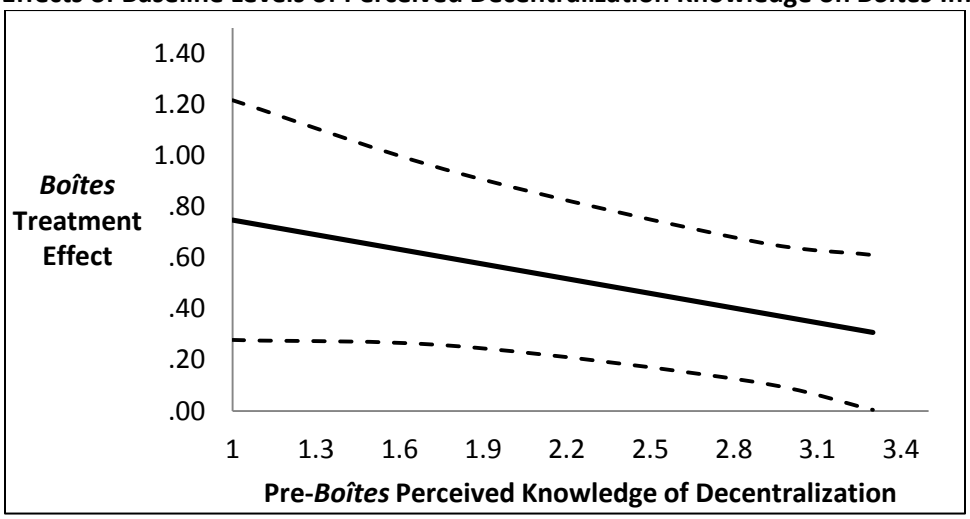
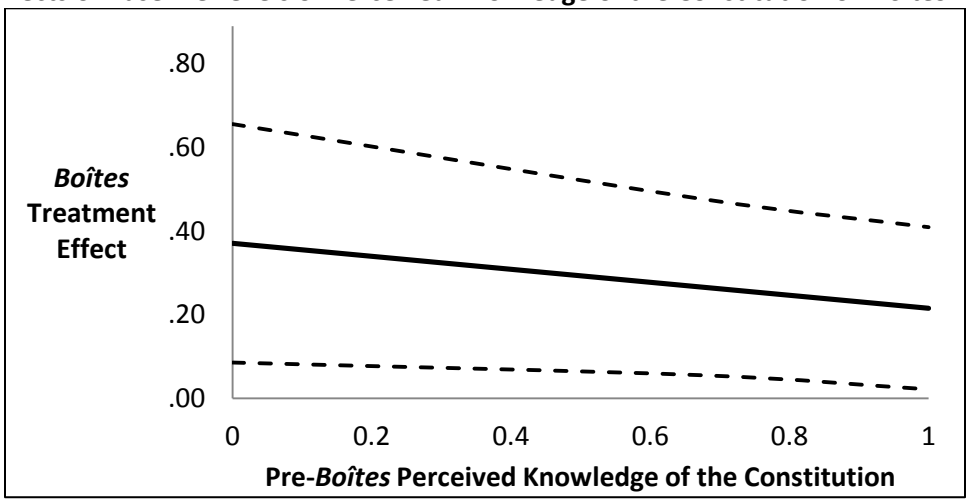


Figure III-17
The Effects of Baseline Levels of Perceived Knowledge of the Constitution on *Boîtes* Impact



The figures show that the *Boîtes* effects are considerable larger among treatment village individuals with lower initial scores than higher. Figure III-15 shows that, among individuals with *no* prior decentralization knowledge, the gains among treatment village individuals are over 1.85 correct answers more than individuals in control villages. This compares to gains of 1.39 for individuals with "average" levels of pre-*Boîtes* decentralization knowledge and gains of only 1

item more for individuals who answered four of the six questions correctly in the baseline interview. While the gains are significant for all individuals, the results do suggest that the effects of the treatment are greater among those with lower initial levels of decentralization knowledge, with the same pattern being found for perceptions of decentralization and constitution knowledge in Figure III-16 and Figure III-17.

It is important to note that these effects are *not* simply due to the well-known "regression to the mean" effect in longitudinal analyses, whereby individuals with lower levels of a variable tend to increase more over time than individuals at higher levels, due to floor and ceiling effects and due to random factors pulling unusually high and low scores back to the "average" at a subsequent time of measurement. These processes are *already* taken into account in the model, as treatment and control village individuals are matched on their initial levels through the CEM process implemented in all models in the analyses. So whatever "regression to the mean" processes exist should be occurring with equal prevalence in both treatment and control villages, and any additional interaction between the treatment and the individual's initial level should reflect a "true" differential effect.

To this extent, the findings support the notion that *Boîtes* effects are stronger among those with greater "need" for the knowledge imparted in the sessions, though this "need" is not necessarily concentrated among individuals in particular demographic groups. Rather, the "need" is based on prior levels of decentralization and constitutional knowledge *per se*, and the sessions appear to have been relatively more successful in stimulating knowledge gains among these individuals in the treatment villages.

Chapter IV. Conclusions

This study was undertaken to determine whether, how, and under what conditions the *Voter Opinion and Involvement through Civic Education Program* (*VOICE*) in the Democratic Republic of the Congo — more specifically, the *Boîtes à Images* sensitization sessions that were the program's core civic education tool — was successful in changing individuals' knowledge and attitudes about the ongoing decentralization process in the country, and in changing more general democratic orientations and political engagement. To accomplish this task, we implemented a complex research design with two components: first, a comparison between respondents from 8 villages in Bandundu province where *Boîtes à Images s* sessions took place and 8 villages from similar "control villages" in neighboring *groupements* where sessions did not take place; and second, a field-experimental "encouragement design" whereby 100 individuals in each of 8 villages were randomly encouraged at the end of a baseline interview to attend the upcoming *Boîtes* session, while 40 randomly-selected individuals in those "treatment villages" received no information about the workshop. Follow-up interviews were conducted with all respondents between one day and one month after the workshops in the treatment villages took place.

The study is one of the first evaluations of adult civic education programs to make use of experimental methods, which provide greater confidence in the attribution of causal effects than has been possible in the mostly passive observational designs in previous research in the field. To our knowledge, it is the first that implements an "encouragement manipulation", one that preserves the benefits of experimentation in a situation where it is not possible to

randomly assign individuals to experience a treatment, nor to restrict exposure to those specifically chosen individuals.

The study points to a series of important findings about the kinds of effects that the *Boîtes à Images* sessions had, and the conditions under which the sessions had their strongest impacts. Some of the findings are unique to the DRC and *VOICE* experience, but some also echo the conclusions that were drawn in the previous evaluations of adult civic education undertaken on behalf of USAID in countries such as Kenya, South Africa, the Dominican Republic, and Poland (Finkel 2003; Finkel and Smith 2011). In this section we first summarize these key results; we then discuss the implications the findings have for the design, implementation, and evaluation of future civic education programs in developing democracies.

A. Major Findings

There were *large* differences between individuals in control and treatment villages in terms of their changes in factual knowledge about decentralization before and after the *Boîtes* events took place in the treatment villages. While respondents in control villages increased to some extent on decentralization-related knowledge (with the percentages of correct responses increasing between 5-10% for most questions), respondents in treatment villages increased at substantially higher rates (between 12 and 45% increases in correct responses for each question). On an overall scale of "correct decentralization knowledge" (ranging from 0 correct to 6 correct answers), the average respondent in control villages increased by only .45 correct answers over time, while the average respondent in treatment villages increased by 1.5 correct answers from the pre- to post-*Boîtes* interviews. The effect is even more dramatic if one examines the proportions of individuals who increased *at all* in their decentralization knowledge over time: in the control villages, just over 1/3 of all individuals (36%) registered some increase; this figure nearly doubles to 69%, or more than 2/3 of all individuals in treatment villages. These effects remains strong in the context of regression models where the treatment and control samples are carefully balanced on possible confounding variables, and in models that control for the individuals' pre-*Boîtes* levels of the decentralization knowledge. These effects are impressive, especially given that they stem from a single civic education event conducted at the village level. To the best of our knowledge, these are some of the largest effects registered in the several adult civic education evaluation studies previously conducted, and certainly the largest registered in these studies on political knowledge (Finkel 2003; Finkel and Smith 2011; Finkel *et al.* 2012). The *Boîtes* sessions do appear to have stimulated substantial gains in knowledge about the decentralization process at the village level from the baseline wave of observation to the post-*Boîtes* interviews.

We also found village-level effects of the *Boîtes* sessions on other knowledge-related items, such as the individual's general knowledge about DRC political institutions and actors, and on the individual's *perception* of their knowledge about decentralization and the DRC constitution. These effects were of smaller magnitude than those observed on the general decentralization knowledge scale, but they do indicate that the *Boîtes* sessions were beneficial to some degree in stimulating *general* political learning, and in stimulating greater confidence among individuals in their own store of decentralization and constitutional knowledge. These findings reinforce the conclusion above that the *Boîtes* sessions appear to have been successful in their immediate goal of transmitting information about decentralization and the DRC political process to their target audiences.

However, there we found few corresponding effects on individual attitudes about, or support for, the decentralization process. There were no differences between the changes in support for decentralization among respondents in control villages and respondents in treatment villages, no differences between treatment and control respondents in terms of their perceptions of the "good" and "bad" features of decentralization, or their satisfaction with the current pace of the decentralization process. Individuals in treatment villages, moreover, tended to change slightly in the direction of greater, not lesser, national authority in a series of policy areas (roads and transportation, schools and education, police and military, taxation), compared to comparable individuals in control villages. There were also no effects of the *Boîtes* session on more general orientations about democracy, on political interest, discussion, or efficacy, nor on political participation. The effects of the *Boîtes* events appear to have been more or less exclusively concentrated in the realm of decentralization and general political *knowledge*, with no concomitant effects on decentralization attitudes or other political orientations or behaviors. These non-effects should be taken into account in the overall assessment of the *VOICE* program's effectiveness; they also provide cautionary evidence against the notion that increasing knowledge about potentially contentious political processes such as decentralization will translate readily into support for those processes among ordinary individuals (see also Green *et al*. 2011).

We exploited the "randomized encouragement" feature of the design in order to determine the direct effects of attending the *Boîtes* event among individuals in treatment villages. Using a variety of "intent to treat" and "instrumental variables" models to overcome possible biases related to self-selected exposure to the treatment, we found consistent evidence that *Boîtes* attendees registered large and significant gains on decentralization knowledge, relative to non-attendees, with these effects being even larger than the overall village effects mentioned above. *Boîtes* attendees also were found to have gained significantly on the other variables (general knowledge and perceptions of decentralization and the DRC constitution, support for national authority in roads, police and military affairs) shown to be significant at the village level as well. Further analysis showed minimal "spillover effects" from attendees to others in their family, neighborhood or friendship networks. Taken together, the results suggest that virtually all of the village-level impacts on the knowledge and decentralization orientations were concentrated among those who attended the *Boîtes* sessions. The sessions appear to have stimulated little, if any, secondary learning due to discussion or spillover from attendees to non-attendees in the village. These non-effects differ somewhat from previous findings of positive spill-over in civic education evaluations conducted in Kenya (Finkel and Smith 2011) and Nigeria (Fafchamps and Vicente 2011). It seems reasonable to speculate that the single, relatively brief *Boîtes* exposure was not sufficient to stimulate the kind of post-treatment diffusion via interpersonal discussion necessary for spillover effects to occur, especially given the breakdown of the decentralization process at the elite level and its relatively low salience in the DRC at the time the study was conducted.

We further examined the conditions under which the *Boîtes* sessions produced larger impacts. Three significant findings emerged. First, the *quality of the Boîtes* session mattered a great deal. Events with higher reported satisfaction of the participants regarding the organization, information delivery and competence of the facilitators showed substantially stronger effects on decentralization knowledge and many of the other knowledge-related items of lesser perceived quality. Importantly, individuals who attended *the highest quality events* often showed significant increases in the cluster of orientations related to political engagement: interest,

efficacy, political discussion and participation, relative to comparable individuals in control villages. The findings echo previous research, where a consistent result has been that higher quality civic education implementation is strongly associated with individual-level effects of greater magnitude. As was found previous research, however, the numbers of individuals in DRC treatment villages who were trained at the highest levels of session quality were relatively small. Thus, the results indicate both the strong *potential* for effects from high quality civic education, as well as the more limited *actual* impacts the sessions had on decentralization and political engagement outcomes.

Second, the *time since the event* mattered as well. Treatment village respondents interviewed closer to a month after the *Boîtes* event showed substantially less improvement in decentralization knowledge, relative to comparable individuals in control villages, than respondents interviewed in days immediately following the event. Treatment effects decline by over 60% from the day after the event to 26 days later on the key variable of decentralization knowledge, with even steeper declines on the other knowledge-related political engagement orientations. This pattern is consistent with a "fade-out" effect, such that individuals are forgetting after several weeks a large portion of what they may have learned in the days following the treatment. We cannot show this conclusively with the data at hand, however, since individuals interviewed at different times after the sessions had been exposed to different treatments. It is also the case that individuals interviewed well after the event report lower perceptions of session quality, making it difficult to disentangle the separate effects of each on the magnitude of the treatment effects. Nevertheless, this pattern shows strong support for the notion — consistent with previous work — that the single-shot *Boîtes* treatments may not have produced lasting impact on the orientations they were designed to affect.

Finally, we found no consistent evidence that the effects of the *Boîtes* sessions differed across individuals in treatment villages with different demographic, political, or social characteristics. There were similar effects of the *Boîtes* session for men and women, for individuals with different educational attainment, for individuals with varying levels of secondary group membership, and for individuals who had and had not lost family members in the recent DRC armed conflict. However, we did find suggestive evidence that the *Boîtes* treatment had greater effects on those with lower initial levels of decentralization and general political knowledge. In every instance, knowledge gains from the *Boîtes* events were greater among those with the greatest "need" for the information. This provides support for a process whereby adult civic education has a "compensation effect" in altering the orientations of individuals with fewest cognitive resources (see also Finkel and Smith 2011).

B. Implications for Civic Education Design, Implementation, and Evaluation

The study's findings and conclusions have definite implications for how civic education programs related to both decentralization and more general democratic orientations should be structured in the future. We make the following recommendations for civic education design, implementation, and evaluation.

1. *Future civic education programs should continue to use "Boîtes à Images" and other visual materials*. The use of these kinds of images and visual appears to have been an effective means for the transmission of basic civics information related to decentralization and democratic political processes in rural populations characterized by relatively low levels of adult literacy.

Moreover, the fact that the effects of the session examined here were *greatest* among those with lowest levels of prior information further validates the image-based approach utilized in the *VOICE* program, given that individuals with a lower store of political information are likely to have lower levels of literacy as well.

2. *Future programs must go beyond "one-off" village-level treatments in order to maximize individual level impact.* It is clear from this study that immediate short-term gains in knowledge about the decentralization and general political process are possible through single-shot civic education treatments. Indeed, as has been noted throughout the report, the magnitude of the knowledge gains on the specific orientation that the *VOICE* program was designed to influence was large and, in comparison to results found in previous research, impressive. At the same time, there were negligible effects of the program on virtually all other orientations studied, and the effects of the sessions themselves appear to have faded significantly over time as well. These patterns conform well to the findings from previous work, where it has been established with a high degree of regularity that *multiple exposures* to civic education treatments are necessary for enduring effects to occur, and that multiple exposures are particularly necessary in altering orientations, attitudes, and political engagement besides factual knowledge. Thus, while we endorse the general form of the civic education delivery in the *VOICE*, future programs *should be strongly encouraged to* build in multiple exposures in order to reinforce the messages to which individuals are exposed and to ward off the "fade out" effects that appear inevitable in their absence.

3. *Future programs must give greater emphasis to the training of facilitators and to improving the general organization of civic education treatments.* Although the *Boîtes à Images* sessions were very popular with most participants, there was nevertheless considerable variation in the satisfaction that individuals registered with the organization of the session, with the quality of the information that was conveyed, and with the competence of the facilitators. And this variation was directly related to the size of the effects observed from the *Boîtes* sessions, with higher quality sessions showing substantially larger impacts than those rated of lower quality. It was also the case that high quality workshops were able to influence more general orientations related to democratic engagement and not "only" stimulate factual learning among participants. But the proportion of individuals exposed to sessions at this level of quality was relatively small, thus inhibiting more extensive effects that could have been observed. We recommend that significantly more resources be devoted to facilitator training and the organization of workshops in the future. "One-off" sessions conducted with inadequately trained and insufficiently knowledgeable facilitators are a recipe for limited individual-level impact, yet large numbers of individuals continue to be trained in these ways in otherwise well-conceived civic education programs.

4. Future programs should emphasize post-treatment discussions of participants to maximize the possibilities of spillover effects. We found little evidence of secondary effects from post-*Boîtes* discussions in treatment villages, and this was a "missed opportunity" in terms of increasing the overall impact of the sessions in important ways. It also differed considerably from the patterns seen elsewhere in Africa, where individuals exposed to civic education went on to discuss their experiences with others, with secondary learning effects seen among those individuals as a result. Civic education programs should exploit this possibility to maximize both the reach and the impact of future activities. Participants in future programs should be encouraged *explicitly* to discuss decentralization and general topics related to democratic

political processes with family, friends, colleagues, and neighbor, and they should be provided with instructional materials in how best to do so. The more individuals are taught how to seek out and discuss civics materials with others both in and out of their immediate social networks, the more future programs will extend their reach and their impact throughout Congolese society.

5. *Future evaluations should continue to exploit experimental designs in order to make more credible causal inferences about program effects, while recognizing their potential limitations in certain settings as well.* The use of the randomized "encouragement manipulation" was a key component of the research design here, and it allowed us to make stronger statements about the effects of civic education exposure, controlling for the self-selection biases that have hindered causal inference in previous work. We recommend that future evaluations incorporate these and other kinds of experimental manipulations to randomize treatment assignment and treatment exposure. At the same time, there were large number of non-compliers with the encouragement manipulation, and especially large numbers of non-encouraged individuals who nevertheless attended the *Boîtes* session. This means that much larger samples of treatment village individuals will be necessary in future research in order to maximize the statistical power of encouragement designs, and to generate sufficient numbers of non-attendees in treatment villages so that more extensive analysis can be conducted on this group. It also means that more efforts should be devoted to validating individual attendance at civic education events in order to provide more objective measures of individual-level exposure. Finally, the success of the experimental manipulations depends to some degree on minimizing discussions about the events between encouraged and non-encouraged individuals. To the extent that large amounts of interactions between encouraged and non-encouraged individuals, and between attendees and non-attendees, it will be that much more difficult to design studies that can distinguish the effects from direct and indirect exposure among individuals in treatment villages at all. We therefore recommend that future evaluations be certain to increase the number of (randomized) treatment and (randomized) control areas so that, regardless of the ease with which direct versus indirect spillover effects can be distinguished, the casual impacts of civic education interventions can be estimated with greater confidence at the overall village level.

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